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A.J. GRAHAM

COLLECTED PAPERS ON GREEK COLONIZATION

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COLLECTED PAPERS ON GREEK COLONIZATION

BY

A.J. GRAHAM

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To Jenny

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INTRODUCTION

When I began serious work on Greek colonization in the 1950s, it was not a very fashionable subject in Greek historical studies and few were pursuing it. That has been completely changed during my professional life. Greek colonization is now, and has been for many years, a highly favoured area, in which many scholars in many countries are active. In part this is a result of the explosion of archaeological work in all the lands where the ancient Greeks established their colonies. Archaeologists have eagerly investigated, not only the colonial sites themselves, but also the indigenous world in which Greek colonists settled, and much new historical evidence is constantly accruing. But the upsurge of interest is also due to the nature and circumstances of Greek colonization, because these involve questions and problems currently engaging the attention of ancient historians. It is a sign of the vitality of the subject that it is generating plenty of controversy.

This book contains all the papers that I have published on Greek colonization during the last forty odd years. Some of them appeared in rather, or even very, inaccessible publications, so it is a pleasure for me that they will all now be widely available, as are my *Colony and Mother City* and chapters on colonization in *Cambridge Ancient History* III,3[2]. Apart from corrections of misprints, the papers are reproduced unchanged, and the original page-numbering is indicated, together with the page breaks, which are marked by a double vertical line.

In addition to the previously published papers, the book also contains one paper not previously published, 'Thasian Controversies', which I completed this year. Since it deals with the reception of two of the papers republished in this volume, its inclusion seemed appropriate.

Finally, I am happy to record my gratitude to my friend and former student, Professor Irad Malkin, who originally suggested the publication of this book.

University of Pennsylvania — October 2000

1. PATTERNS IN EARLY GREEK COLONISATION

The centuries before the Greeks began to write history are of the greatest interest and significance, but they are also for the historian full of uncertainty, obscurity and dispute.[1] The reason is mainly the simple inadequacy of the information with which the historian has to work, and it is partly because of that inadequacy that the colonising movement[2] is uniquely valuable. Part of this value lies in the revealing nature of colonial activity in itself. Think what it tells us about geographical knowledge, seafaring, if not seapower, military achievements, state organisation, economic conditions—to mention only a few broad and obvious categories. But in a period so inadequately known the colonising movement is also extremely important to the historian as a source of a relatively large amount of clear and unequivocal facts. In the context of the eighth and seventh centuries BC the fact that such and such a city sent a colony to such and such a place constitutes a rare piece of definite and valuable knowledge. Since it is also possible to assign dates to large numbers

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[1] This is substantially the text of a lecture delivered to the Hellenic Society on March 18, 1971. I am most grateful for helpful suggestions made by some of my auditors on that occasion.

In this paper I refer to the following works by the author's name alone (or as indicated):

J. Bérard, *L'expansion et la colonisation grecques jusqu'aux guerres médiques* (Paris 1960).

J. Boardman, *The Greeks Overseas* (London 1964).

G. Buchner in *Metropoli e colonie di Magna Grecia* (*Atti III Convegno di studi sulla Magna Grecia*, Naples 1964) 263-74 (Buchner, *Convegno*).

G. Buchner, "Pithekoussai, oldest Greek colony in the West" in *Expedition* viii no. 4 (1966) 4-12 (Buchner, *Expedition*).

J. N. Coldstream, *Greek geometric pottery* (London 1968).

R. M. Cook, 'Ionia and Greece in the eighth and seventh centuries BC' in *JHS* lxvi (1946) 67-98.

T. J. Dunbabin, *The western Greeks* (Oxford 1948).

M. J. Mellink (ed.), *Dark ages and nomads* (Istanbul 1964) (*Dark ages and nomads*).

E. Meyer, *Geschichte des Altertums* (2nd ed. Stuttgart 1937) iii 388-438.

C. Roebuck, *Ionian trade and colonization* (New York 1959).

[2] There are no recent, complete and fully documented histories of the great Greek colonising movement of the archaic period. The nearest approach to such a work is Bérard's, but it is most unfortunately incomplete (see my review, *JHS* lxxxi [1961] 201). Boardman's book is extremely useful as a comprehensive account of the archaeological evidence.

Journal of Hellenic Studies 91 (1971), 35-47

of colonial expeditions, we can say that the colonising movement provides a large amount of extremely significant historical information, which is in general terms clear and definite and relatively well dated.

There is still a further reason for the general importance of the colonial movement. This is a field in which we can look for continuing improvement in our knowledge. Apart from new fragments of the early lyric poets on papyrus the only likely source of new information on the history of these centuries is archaeology, and the history of the colonising movement is especially well suited to benefit from archaeological discoveries. It would be fair to say that much of the most important new material on early Greek history has come from archaeological discoveries on Greek colonial sites. The epoch-making excavation of Old Smyrna not only transformed our knowledge of the Greek settlement of the coast of Asia Minor, but also contributed most significantly and valuably to the question of the nature and rise of the Greek city-state.[3] The discoveries at Al Mina in Syria have not only thrown light on Greek trade and colonisation in very early times, but have also effectively determined our interpretation of the relations between the Greeks and the East and the oriental influences on early Greek civilisation.[4] On the island of Ischia the excavations of Pithecusa have provided on the one hand invaluable information about the earliest Greek ventures in the West, but on the other the site is one of the most informative of all Greek settlements of the || eighth century.[5] At Metapontum it is possible to see in air photographs how the city's land was divided. This division can be shown to belong to archaic times, even if it does not represent the original division made at the establishment of the colony at latest in the early seventh century.[6] We thus have a most significant addition to our knowledge about land division in Greek settlements, which is also evidence which throws light on Greek society at the

[3] J. M. Cook, *BSA* liii-liv (1958-9) 1-34; *The Greeks in Ionia and the East* (London 1962) chs. 2 and 3; *CAH* ii^2 ch. 38.

[4] T. J. Dunbabin, *The Greeks and their eastern neighbours* (London 1957) chs. 2-5, especially 25-9; Boardman ch. 3, especially 61-74; Coldstream 310-16, 345, 384-5, 423.

[5] See the valuable general accounts of his excavations by Buchner cited in n. 1 above.

[6] Adamesteanu, *Rev. Arch.* 1967 3-38, especially 25-7. *Cf.* A. Giuliano, *Urbanistica delle città greche* (Milan 1966) 44-5, 48, whose arguments for the highest possible date are not completely convincing.

time, and even on such important themes in Greek history well known from later times, like redistribution of land and equal rights.[7] Not that I want to fall into the common error of overestimating what archaeological evidence can tell us. The literary sources on Greek colonisation may be small in bulk, but they will always remain the most important. Archaeology cannot provide us with the precise and detailed knowledge we often obtain from a literary account. Cyrene will, I imagine, always be the best known of early Greek colonial enterprises—because of Herodotus not the spade.[8] But even when we ask the archaeological record the relatively simple questions which it is capable of answering there are still dangers in the use and interpretation of archaeological evidence about Greek colonies. Even the establishment of the fact of settlement at a certain date is not entirely straightforward. How much material proves a settlement? At Acragas, where we have good literary evidence, the finds include a little material of earlier date than 580. Yet it seems wrong to reject the literary foundation date, since the bulk of the material comes from after that time.[9] A clear example, it appears, that the earliest material on a site may not date the settlement. At Thasos, on the other hand, no archaeological finds are earlier than about 650. Yet I doubt if many would care to put the foundation of the colony so late.[10] This illustrates a major difficulty which is always present: can one be sure that the earliest material has come to light? In addition to these fundamental problems, we have the uncertainties of interpretation which always arise when the main evidence—as is normally the case—is painted pottery. Once pottery is an object of trade, and not simply a personal chattel locally produced, its presence on a site need not tell us anything about the movements of the people who made it.[11] To illustrate the point by absurdity: it has been pointed out that if we had no literary evidence for the origin of the settlers of Pithecusa, and if we had no records of their writing, we

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[7] On this topic see D. Asheri, *Distribuzioni di terre nell' antica Grecia* (*Mem. Accad. Sci. Torino, Classe Sci. Mor. Stor. Fil.*, ser 4a no. 10, Turin 1966) chs. 1 and 5.

[8] F. Chamoux, *Cyrène sous la monarchie des Battiades* (Paris 1953) ch. 3; Boardman 169-73.

[9] Boardman 198-9; Dunbabin 305-12.

[10] Boardman 238; *cf.* J. Pouilloux, *Recherches sur l'histoire et les cultes de Thasos* (Paris 1954) 22-3.

[11] *Cf.* my remarks and references in *Colony and mother city in ancient Greece* (Manchester 1964) 13-14.

should be tempted by looking at the eighth-century pottery to call it a Corinthian colony.[12]

When we consider all these difficulties we must concede that the archaeological evidence cannot totally change the nature of our knowledge of Greek colonisation. That knowledge will always remain on the whole skeletal, a framework of simple facts about origins and dates only rarely enriched by detail. The picture of Greek colonisation that we can achieve will, therefore, always be drawn in rather broad lines, and the task of the historian is continually to try to improve the quality and validity of these rather general reconstructions. In that task he inevitably works not just with the direct source material but also with the background or the context within which colonial expeditions were made. In simple terms this activity is looking at patterns on a map; hence the justification for my title. If we look at the pattern of Greek colonies on a map, we find, for instance, that Egypt is virtually free of Greek colonies, and it is a commonplace to conclude that the Greeks were not able to || establish settlements in the territory of an advanced and organised state. As it happens, even here the pattern is not so simple. The colony at Naucratis, a trading post established by permission of the Pharaoh, and existing under precise Egyptian regulations,[13] might seem merely to emphasise that Egypt as a whole was not Greek colonial territory. But the very large-scale permanent settlements of Greek mercenary soldiers in Egypt, as has been well observed recently,[14] can be seen as a kind of colonisation, in which the need of the Greeks to settle abroad was specially adapted to the opportunities available in an advanced host country.

It is not surprising that the interpretation of the patterns of Greek colonisation generally becomes more satisfactory as we descend in date. It is, for example, clear that we can combine what we learn from literary sources, especially Herodotus, with archaeological evidence, to obtain a generally valid picture of the Phocaean colonisation in the Western Mediterranean from the end of the seventh century.[15] The chronology, causation and character of this colonisa-

[12] Buchner, *Convegno* 267.

[13] See the very good discussion by M. M. Austin, *Greece and Egypt in the archaic age* (*Proc. Camb. Phil. Soc.* Suppl. 2, 1970) 22-23.

[14] Austin *op. cit.* 15-22, especially 18.

[15] *Cf.* Boardman 223-30; Bérard 129-33.

tion emerge clearly enough; we have the attractions of the riches of Spain, good relations with native peoples both in Spain and Gaul, the ability of the Phocaeans to compete with hostile rivals, especially the Carthaginians, the pressure of oriental powers on the mother city. This relatively rich and secure evidence of all kinds is not fully matched for the Black Sea colonies, but even so the pattern of settlement there, from the latter part of the seventh century onwards, is also reasonably clear.[16]

In both these areas and periods archaeological evidence is now available to supplement or to test the literary information. That is not yet true of all areas and periods of Greek colonisation. The absence of archaeological evidence does not prevent pattern-making, as can easily be seen by consulting any pre-archaeological account of Greek colonisation. There is a good account of this nature in Meyer's *Geschichte des Altertums*.[17] A combination of literary evidence, geography, knowledge of the history of non-Greek peoples and shrewd estimates of probability yields patterns which are not by any means totally different from those we should draw today. But one notes above all the way in which an author without archaeological evidence has no way of checking or confirming his literary evidence. He is also forced to turn frequently to the very dubious evidence of myths in order to enrich his picture.

When one turns from such pre-archaeological accounts to R. M. Cook's very useful discussion of Greek colonisation in his paper of 1946,[18] it is easy to see the great improvement in the strength and quality of the historical reconstruction which the archaeological evidence has allowed. This was a very well-informed and well-balanced critical account of what was known of the Greek colonising movement at that time, and it makes a good base from which to estimate the improvements which have accrued in the last quarter of a century. In addition Cook's remarks about the ways in which reconstructions of the history of Greek colonisation have been made

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[16] Boardman 255-67; Bérard 100-7; Roebuck ch. 8. The active archaeological work on the Greek colonies in the Pontus is mostly published in Slavonic languages. There are useful accounts of that work by Boardman, *AR for 1962-3*, in English, and E. Belin de Ballu, *L'histoire des colonies grecques du littoral nord de la Mer Noire* (Leiden 1965), in French. It is to be hoped that the results of the recent work on the archaeological evidence for Greeks in the Pontus by Dr J. G. F. Hind of the University of Otago will become generally available through publication.

[17] See n. 1 above.

[18] Pp. 70-80 (see n. 1 above).

provide an enlightening text for what I am attempting in this paper. He notes that people have tended to use—to a greater or lesser degree—a simple geographical determinism (leading in its most extreme form to the thesis that the nearer the colony to the founding state the earlier the date of foundation), and have ignored such factors as "the comparative attractiveness of sites, the attitude and strength of the native inhabitants, and chance: but these are factors of which we know little."[19] We may admit ll the truth of the last comment, but since the factors mentioned may well have been decisive in most, if not all, colonial enterprises, no reconstruction which ignores such aspects can be called satisfactory.

One of the results of the possession of the new evidence from archaeology has been a tendency to be unwilling to trust the literary evidence when it stands unconfirmed by archaeological discoveries, a tendency which we can also see in Cook's paper. Such an attitude is no doubt from an ideal point of view perfectly correct. We must all look forward to the day when the literary evidence can all be evaluated against a background of adequate archaeological exploration. But it seems to me that the outstanding general result from all the archaeological discoveries about Greek colonisation has been that the literary record is on the whole thoroughly trustworthy. This conclusion has, it is true, been more solidly established by evidence unearthed since Cook's paper was written. At that time there was a disturbing element of circularity in the argument. The painted pottery had been given absolute dates from the literary foundation dates of the colonies in the West. It was not, therefore, entirely satisfactory to go on to conclude that the pottery confirmed those literary dates. But the excavations on Ischia have provided external dating evidence—notably the Bocchoris scarab—so that the chronology of the painted pottery can now be regarded as independently established.[20] As a result we now have in Italy and Sicily enough good archaeological evidence even for the colonies of the eighth century to be able to assess the quality of the literary sources. The result is highly encouraging, perhaps even a little surprising. Was it entirely to be expected that a sentence of Livy[21] standing alone and referring to events of the eighth century BC would be so triumphantly

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[19] 70 with n. 28.
[20] Coldstream 316-17, 322-7.
[21] viii 22.5-6.

justified as it has been by the excavation of Pithecusa? It is important to note here that the modern arguments about the chronology of the early colonies in Sicily do not seriously shake the general reliability of the literary dates. However one may believe the authors of the fifth century and later obtained the foundation dates which they transmitted—whether or not they were artificially computing by generations—the fact is that those dates have been proved to be in general terms reliable.[22]

This is a result of major importance for early Greek history in general, but for my present purpose I would emphasise its implications for the history of Greek colonisation. It shows that when the Greeks came to write history they were able to discover the fundamental facts about many colonies established in the eighth century, not just the origin of the colony but also its date. It seems therefore that it is not good method to ignore or disbelieve information about early colonial foundations in our literary sources merely because that information is without archaeological confirmation. Thus, if we venture to look at patterns in early Greek colonisation in areas where the archaeological evidence is still very slight, we have at least this advantage over our pre-archaeological forerunners: we know that the literary evidence on which we mainly depend, and in particular the literary foundation dates, have been proved in other areas to be in general reliable.

In the light of these introductory remarks it seems worthwhile to reconsider certain aspects and areas of Greek colonisation in the eighth and early seventh centuries, where there is manifestly still room for improvement in our understanding of the general pattern. (Thus I exclude Sicily, where a relatively satisfactory picture, even of the earliest colonisation, can be said to have been achieved.)[23] In some of these areas new archaeological evidence acquired over the last two decades has made such a reassessment necessary. In others, although there is no such direct new evidence, the implications of the new evidence from elsewhere affect our interpretation of the existing historical sources. In attempting || this reconsideration I also

[22] Coldstream *ibid.*; Dunbabin Appendix I; my *Colony and Mother City* 221 n. 2.

[23] See, for instance, the good accounts of Bérard, *La colonisation grecque de l'Italie méridionale et de la Sicile dans l'antiquité* (2nd edition Paris 1957) chs. 2. 3, 6; Dunbabin chs. 1, 3; A. G. Woodhead, *The Greeks in the West* (London 1962) ch. 3.

hope to give due weight to those factors, which Cook recognised to be important, but then dismissed because they were so little known.[24]

The little eighth-century colonisation in the Black Sea which is attested by literary sources has still neither been confirmed nor disproved by archaeological evidence, so I have nothing to add to what I wrote about this subject some years ago.[25]

The colonisation of the Propontis[26] offers a promising field for speculation in the attempt to make sense of the pattern of settlement. It has long been held, for instance, I am sure correctly, that the absence of Greek colonies from the north shore in early times is to be attributed to the warlike strength of the local Thracian tribes.[27] There are several examples in the history of Greek colonisation of expeditions which fell foul of these dangerous opponents.[28] They may also be called in to explain the paradox, which has troubled interpreters since ancient times,[29] that Chalcedon was settled before the magnificent site of Byzantium. We need an explanation, for the splendid headland of Byzantium is an example of a type of site particularly favoured by Greek colonists.[30] They would not have rejected it in favour of Chalcedon without some very strong reason.

However, if the north side of the Propontis was largely forbidden territory for the earliest Greek colonists, we find a number of colonies on the south side with early foundation dates in the literary sources. Eusebius gives the following foundation dates: for Cyzicus 756 and 679; for Astacus 711; for Parium 709; for Chalcedon 685 (and Byzantium 659).[31] Herodotus' story (iv 14-15) of Aristeas of Proconnesus implies that Cyzicus and Proconnesus were already in existence as Greek cities more than 240 years before Herodotus' own time, say *c.* 690.[32]

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[24] See above p. 37.

[25] *BICS* v (1958) 25-42.

[26] For recent useful accounts and discussions see Bérard 95-100; Boardman 245-55; Coldstream 376-80; Roebuck 110-15.

[27] See, for instance, already Meyer 418.

[28] As Thasos, see Pouilloux, *Recherches sur l'histoire et les cultes de Thasos* 22-3; Abdera, see Hdt. i 168; and, later, Amphipolis, see Thuc. iv 102.2-3.

[29] Hdt. iv 144.1-2.

[30] Examples of such sites: Croton, see Dunbabin 85 opp.; Bérard, *Colonisation . . . l'Italie . . . et . . . Sicile* 157; Elea, see Bérard *op. cit.* 270-1; W. Hermann, *AA* 1966, 360-4; Istrus, see *BCH* lxxxii (1958) 337-8 figs. 2 and 3.

[31] R. M. Cook usefully set out the literary dates of colonial foundations (77).

[32] As Cook 77; *cf.* 71 n. 42.

There is a great dearth of archaeological material from this area, and the only site which has been excavated and yields material of a relevant date is the modern Hisartepe, a place some 20 miles south of Cyzicus. Here part of an entirely Greek city with pottery as early as the first half of the seventh century was unearthed.[33] The site was with great probability identified by its excavator as Dascylium, capital of the third Persian satrapy.

In attempting to reconstruct the pattern of early Greek colonisation in this region we need first of all to set this small quantity of direct evidence in its geographical context. The Propontis has been well called 'a little Aegean'.[34] Other things being equal we should expect it to be a very attractive area for Greek colonists. Fortunately we can make estimates as to how far other things were equal by using our knowledge of non-Greek peoples, whose activities can be shown or assumed to have influenced the colonising movement. The three peoples who come into the picture are the Cimmerians,[35] the Phrygians and the Lydians.

R. M. Cook made himself merry about those who used the Cimmerians—"these dim but useful barbarians"—in order to explain problems in the record of Greek colonisation.[36] And it is still true that archaeologically speaking the Cimmerians remain very fugitive. There is no occupation level at any site which can be securely identified as theirs.[37] But a people who are firmly attested in contemporary oriental and Greek records[38] cannot be simply ignored, so that we are required to take account of their possible activities and the effects which these may have had on colonial enterprises in the regions under discussion. Such theories are not, after all, pure modern speculation. Of two Greek sites—Sinope in the Black Sea and Antandrus in the Troad—our literary sources actually state that there was Cimmerian occupation,[39] and the widespread destruction by Cimmerians in Asia Minor is indisputable.

[33] Akurgal, *Anatolia* i (1956) 15ff.; *cf.* Coldstream 377; Boardman 249, 254.

[34] *CAH* iii 658.

[35] The Cimmerians were not the only nomadic people in the regions under discussion at this time; see *CAH* iii 187-9, 511; but since we cannot distinguish them convincingly I shall use the term Cimmerians to embrace all the nomads active in Asia Minor in the eighth and seventh centuries.

[36] 73.

[37] Cf. *Dark ages and nomads* 63.

[38] See *CAH ibid.*; Barnett, *CAH* ii^{2} ch. 30, 11-12; U. Cozzoli, *I Cimmeri* (Rome 1968) ch. 8.

[39] Sinope: Hdt. iv 12.2, Ps-Scymnus 941-53; Antandrus: Aristotle, see Steph. Byz. s.v.

We know that Cimmerian and possibly other nomadic raiders were active in Asia Minor over a period of some fifty years from about 700. Their most famous destructions, of Gordium, for instance, or Sardis, show their strength. There is nothing in the least improbable in the theories that certain Greek colonies, founded before their incursions, were destroyed by them and then subsequently re-established when the raiders had been defeated and disappeared. This is what we are expressly told happened at Sinope (in the long account of Ps-Scymnus), and it seems to me a perfectly acceptable way of explaining the two dates for Sinope's foundation, one in the eighth century, by implication, and one in the seventh, which we have in our literary sources.[40] So I also see no theoretical objection to the similar explanation which has been offered for the two foundation dates of Cyzicus in the literary record.[41] The Cimmerians destroyed Gordium and the Phrygian Empire early in the seventh century.[42] Cyzicus could well have suffered a similar fate, possibly even at the same time.

It also seems an acceptable notion to me that the dangerous if sporadic appearances of the nomadic raiders made the whole area of Pontus and Propontis relatively unattractive to Greek colonists in the first half of the seventh century.[43] When the people of Colophon decided to settle elsewhere under pressure of the Lydians, probably early in Gyges' reign, they chose to colonise Siris in southern Italy, the only early colony in the West founded from Ionia.[44] Their choice certainly suggests that all the geographically more accessible Pontic and Propontid region was unattractive to them for one reason or another, and the Cimmerians may well have furnished one of those reasons.

The great period of Phrygian domination was the second half of the eighth century, and it came to an abrupt end with the Cimmerian destruction of Gordium at the beginning of the seventh.[45] It is virtually certain that the territory on the south side of the Propontis

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40 *BICS* v (1958) 33-4 with n. 15, where I discussed modern rejections of Ps-Scymnus' information, 941-53.

41 E.g. N. G. L. Hammond, *History of Greece to 322* BC 115.

42 Young, *Proc. Amer. Philos. Assoc.* cvii (1963) 351.

43 *Cf.* Cook 79 n. 108: "possibly that field was not then ripe for colonisation."

44 For references and discussion see Dunbabin 34-5; Bérard, *Colonisation . . . l'Italie . . . et . . . Sicile* 187-98; *cf.* Boardman 195; Cook *ibid.*

45 *Cf.* Young, *Dark Ages and Nomads* 55.

will have been under Phrygian rule at this time.[46] There were three Greek colonies on this territory in the period in question according to our literary sources: Cyzicus (756), Astacus (711) and Parium (709). The high date for Astacus, a Megarian colony, has long been doubted.[47] Our earliest authority on the colonisation of the area, Charon of Lampsacus, calls Astacus a colony of Chalcedon.[48] It should therefore apparently be dated after 685, the foundation date of Chalcedon. Possibly the higher date of Eusebius is to be explained as a result of calculations designed to glorify the antiquity of the predecessor of Lysimachus' new foundation of Nicomedia.[49] If we leave Astacus on one side, we have one colony, || Cyzicus, allegedly settled about the beginning of the high Phrygian period, and another, Parium, near its end. In view of the intrinsic attractions of the Propontis for colonists, we might be justified in supposing that Phrygian rule on the whole excluded Greek colonisation.

From such a conclusion it is a short step to doubting the literary sources and denying the high dates for Cyzicus and Parium. Perhaps we need not press the matter in the case of Parium. Its origins are somewhat confused in our sources,[50] and its traditional date is near enough to the destruction of the Phrygian Empire for it to be easy to accept the possibility of a small overestimate and assume that it was in fact founded after the fall of that power. But the first foundation date for Cyzicus must either be rejected outright, or taken to show that a Greek colony could exist on that site in the high Phrygian period.

Earlier writers did not find the latter assumption difficult. Meyer thought the Phrygians might have taken their alphabet from the Greek colonies in the Hellespontine area.[51] The discovery that the Phrygians were using the adapted Phoenician alphabet by *c.* 725 has led to the suggestion that they may not have received it through Greek intermediaries at all.[52] But the argument from chronology is not compelling,[53] and since we have good evidence for contacts

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[46] See *RE* s.v. 'Phrygia' 787-8.
[47] It is rejected by Meyer 419 n. *Cf.* Bérard 96-7; K. Hanell, *Megarische Studien* (Lund 1934) 119-22.
[48] *FGH* 262 *fr.* 6.
[49] *Cf.* Hanell's views about the various foundation legends, *op. cit.* 120.
[50] Bérard 97; F. Bilabel, *Die ionische Kolonisation* (*Philologus* Suppl. xiv) 49.
[51] 423.
[52] Young, *Proc. Amer. Philos. Assoc.* cvii (1963) 362-4; cf. *Dark ages and nomads* 55.
[53] *Cf.* the useful discussion by Coldstream 379-80.

between Greeks and Phrygians[54] it seems an unnecessary extra hypothesis to suggest that the Phrygians also obtained their alphabet by contacts in the Phoenician area.

However, evidence for peaceful intercourse between Greeks and Phrygians does not prove that the Phrygians would have allowed Greek colonists to settle on territory that they controlled. To my mind the general absence of colonisation in both Pontus and Propontis in the second half of the eighth century most probably implies that they did not. But this conclusion does not necessarily carry with it the rejection of the first foundation date of Cyzicus. It is possible to envisage the foundation of Cyzicus as preceding the full power of Phrygia. It is worth emphasising that similar early dates are attested for the first colonies in the Pontus.[55] We should also remember that Cyzicus was almost certainly then an offshore island.[56] It seems possible to retain confidence in the first foundation date for Cyzicus while at the same time accepting the view that at its height the Phrygian Empire effectively prevented new colonial settlements on the south shore of the Propontis.

We may begin our examination of Lydian influence on the Greek colonisation of the Propontis with the very interesting piece of information that Milesian Abydus was founded with Gyges' permission.[57] The credibility of this statement has been impugned,[58] but we must surely concede that the specially good relations we know to have existed between Miletus and the Lydian kings could well have led to privileges in the field of colonisation.[59] Recently, however, a much more extreme interpretation of the information about Abydus has been offered. It has been suggested that it was in fact a settlement of Greek mercenaries in Lydian service, and that the Greek settlement which has been discovered south of Cyzicus, and identified as

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[54] Roebuck 43-7; *cf.* Boardman 104-9.

[55] See *BICS* v (1958) 25-6.

[56] F. W. Hasluck, *Cyzicus* (Cambridge 1910) 2-5. I now regard his arguments as convincing; contrast my statement on p. 39 of the article cited in the previous note, though the main argument is not affected.

[57] Strabo xiii 590.

[58] Cook thought (71 n. 41) that the connection with Gyges might have been invented at a later date because a promontory near Abydus was called Gygas (Strabo *ibid.*), but it seems just as easy to accept the more straightforward interpretation implied by Strabo.

[59] Cf. *CAH* iii 508.

Dascylium, should be interpreted in a similar way, because Dascylium was called after Dascylus, Gyges' father.[60]

Boardman puts forward this view in explicit opposition to the conclusion which I drew from the discoveries inland from Cyzicus—namely that a Greek settlement twenty miles || inland from Cyzicus implies that the Greeks had already established themselves on the south shore of the Propontis. Boardman's line is that if the settlement identified as Dascylium is a settlement of Greek mercenaries in Lydian service it need not imply anything about Greek colonisation in the Propontis. I would only reply, firstly, that I do not know of any clear evidence that the Lydians did establish such settlements of Greek mercenaries, and, secondly, the argument from the fact that the site was called Dascylium is open to the objection that there were several places of that name in the Asia Minor region, and the name has been thought by linguistic experts to be a local formation, i.e. not commemorative of an individual.[61] It still seems best to me, therefore, to conclude that on the basis of what we know about Greek colonisation in general a settlement inland most probably implies previous settlement on the coast,[62] and to see the Greek city identified as Dascylium as evidence that it was possible for Greeks to penetrate inland in this area after the fall of the Phrygian Empire.

It follows that I should also prefer simply to accept what we are told about Abydus as it stands, and to adhere to the view that one may infer from it that colonising activity in the region went on with the blessing of the Lydian king. Such a hypothesis makes it easier to understand the dominating position of Miletus in the colonisation of the Propontis (as also in the Pontus), and the extreme paucity of colonies founded by other Greek cities in this readily accessible and attractive area. The first Megarian colony, if we may follow Charon of Lampsacus,[63] is right at the far end of the Propontis at the not outstandingly inviting site of Chalcedon, and one recalls again the Colophonian colony at Siris in southern Italy. We have suggested that these colonists went to the West as the East was generally unattractive at that time because of the Cimmerian danger. I believe

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[60] See Roebuck 112 for the original suggestion, accepted and developed by Boardman 249, 254.

[61] *RE* s.v. 'Daskyleion' 2219 (Ruge); Sundwall, *Klio* Beiheft xi (1913) 63, 194; Bilabel, *Die ionische Kolonisation* 45.

[62] As J. M. Cook, *The Greeks in Ionia and the East* 51; also Coldstream 377.

[63] See above p. 40.

that to be true, but there may also have been a special reason. Impelled to colonise by pressure from Gyges, they might well seek to avoid an area in which he had influence.

To sum up on the early Greek colonisation of the Propontis. Apart from Cyzicus, and perhaps Proconnesus and Parium, the first major effort was by the Megarians at the further end. So in addition to the long-lived difficulties of the north shore the southern shore too was not readily available. This is most easily explained by, first, Phrygian power, then the danger of the nomad invaders, and, finally, some Lydian control. In such a picture I have suggested that the first foundation date of Cyzicus need not be rejected. This is clearly the most hazardous part of the proposed pattern. But it does not seem to me difficult in principle to envisage Greek colonisation of Cyzicus about the middle of the eighth century. Shortly before this Greeks began to settle in the Troad, if we may judge from the beginning of Greek Troy,[64] and this is a time of increased seafaring and trade in general,[65] as well as being the time of the first colonisation in the West. But such questions will inevitably remain to some extent at the mercy of subjective judgment as long as the Propontis is virtually unexplored archaeologically. It is worth pointing out that the sort of things we badly need to know about colonisation in the Propontis, such points as chronology in general, or whether there are any violent destructions, are the kinds of questions to which archaeology is well-equipped to provide answers. I have tried to show that one can profitably look for patterns here in spite of the shortage of direct archaeological evidence, but it is also clear that even a small increase in such archaeological evidence could greatly improve our knowledge of Greek colonisation in the Propontis.

In the West, as we have seen, the situation is entirely different, mainly because archaeological evidence is abundant. As a result, I shall confine my attention to one old problem in ll the pattern of the early Greek colonisation in Italy and Sicily: why is it that the earliest Greek colonies, Pithecusa and Cumae, are also the most distant from Greece? There could be no clearer warning that in the interpretation of the pattern of Greek colonisation geographical determinism is not enough. At first sight it seems to offend against commonsense that the earliest colonists should have rejected and sailed

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[64] *Cf.* Boardman 101-2, 248; Coldstream 376-7.

[65] Well emphasised and illustrated by Coldstream 335, 357-9.

past all the sites which later supported flourishing Greek cities. However, not all ancient historians have thought that any special explanation was required. Meyer thought that it was merely that the Bay of Naples was the most attractive place of all, and the first colonists had first choice,[66] an explanation which seems to be too simple to be convincing. A more recent suggestion, though still before the excavations on Ischia, was Dunbabin's: that it was trade with Etruria, and especially the lure of metals, which drew the first colonists so far from home.[67]

The discussion has certainly been put on an entirely different plane by Buchner's invaluable excavations of Pithecusa.[68] In the first place they have made it quite certain that this is indeed the earliest Greek colony in the West. The foundation date is put in the second quarter of the eighth century. But in addition they have also thrown interesting light on the character of the settlement. It is clear on the one hand that in the eighth century objects from Greece, the eastern Mediterranean and Egypt, not to mention the Italian regions nearer at hand, found their way to the colony in large quantities. We thus have a picture of a community settled far away from its original home which maintained widespread commercial contacts.

In addition the excavator has found unequivocal evidence for the smelting of iron at Pithecusa—namely quantities of iron slag, terracotta bellows-mouthpieces, and two bottoms of coarse pots which are encrusted with iron slag, i.e. they had served as crucibles. This evidence for iron-smelting comes unfortunately from an unstratified context, a dump consisting of material extending in date from the Bronze Age to the second century BC, but the fortunate appearance of a similar piece of iron slag in a purely eighth-century level in the cemetery has justified the assumption that iron-smelting was practised in eighth-century Pithecusa. The origin of the iron-bearing rock that the Pithecusans were smelting is thought to be Elba, so that the colonists of Pithecusa are envisaged as bringing the stone in ships from Elba in order to smelt it at home.[69]

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[66] 439.

[67] Dunbabin 3, 7-8; for a different view, see R. M. Cook, *Historia* ix (1962) 113-14.

[68] For Buchner's general accounts of his results see n. 1 above.

[69] Buchner, *Expedition* 12; *AR for 1966-7* 30; Ridgway, *Studi Etruschi* xxxv (1968) 318-19. I am also indebted to Dr Ridgway for valuable verbal information on this topic.

On the basis of these discoveries it has been claimed that Dunbabin's view about the motives for the foundation of Pithecusa has been triumphantly vindicated. The settlers at Pithecusa are seen as living by trade and industry. They obtain iron-bearing stone and perhaps other metals from the Etruscan region and pay for it with manufactured products and luxuries from the civilised peoples of the eastern Mediterranean. The commercial connections in the eighth century between the Euboean cities and the East are attested by evidence from Al Mina and elsewhere. The Euboean colony at Pithecusa is therefore to be seen as an emporion, settled in order to facilitate and exploit a trade between the eastern and western Mediterranean which, it is suggested, was at that time in the hands of the Euboeans. In support of this interpretation it is pointed out that Ischia is not good terrain for grain production—even if its soil is rich and suitable for olives and vines—so that one would not expect the site to be chosen for an agricultural settlement.[70]

Have we an example here of the way in which an old problem can be solved by new evidence from archaeology? If I would answer, perhaps not yet, or not completely, it is because I see a number of difficulties in the way of accepting the interpretation I have just outlined. ||

The first of these difficulties concerns iron and iron-working, and it is therefore necessary to have a clear picture of the general situation with regard to iron resources and iron-working in the Greek world of the eighth century.[71] This is all the more necessary since there are some strange but widespread misconceptions about such matters which frequently disfigure the works of archaeologists and ancient historians.

Iron-bearing rock is one of the commonest formations.[72] In the Greek world such iron-bearing rocks are found in a large number of places.[73] The islands of the Cyclades are particularly rich in iron

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[70] See the discussions by Buchner and Ridgway see cited above (nn. 1 and 69).

[71] On the subject in general see R. Pleiner, *Iron-working in ancient Greece* (Prague 1969); R. J. Forbes, *Metallurgy in Antiquity* (Leiden 1950) ch. 11; H. H. Coghlan, *Notes on prehistoric and early iron in the old world* (Oxford 1956). I would express here my gratitude to Professor F. C. Thompson, Emeritus Professor of Metallurgy, University of Manchester, for his kindness in discussing this topic with me and for valuable advice.

[72] Forbes 380; Coghlan 13. Forbes' map, fig. 80, showing only the most important deposits of the Near East, illustrates the point graphically.

[73] For deposits in Greece see Pleiner 23-4; *RE* Suppl. iv 117-18; C. Neumann and J. Partsch, *Physikalische Geographie von Griechenland* (1885) 229-35.

resources, but so is the mainland. Euboea itself had plentiful deposits.[74] As a result iron was widely manufactured locally in historical times and no one centre predominated.[75]

The ancient method of smelting iron requires merely three things: the iron-bearing rock, a supply of fuel, i.e. wood for charcoal, and the necessary metallurgical skill. Once they had the last, there were numerous Greek communities which could manufacture their own iron. Although there may be room for argument as to the precise date when the Greeks entered into the full Iron Age, i.e. the time when iron was regularly used for all the tools and weapons for which it is best suited, there is no doubt that that date precedes the eighth century. Iron was certainly widely used by the tenth century.[76] It is true that widespread use does not necessarily prove that they were producing it locally, and there is, to my knowledge, no definite evidence to prove that the Greeks were smelting iron as early as this.[77] It is *a priori* probable since the knowledge of smelting was already centuries old in Asia Minor, and it has therefore been assumed that there was local production in Greece from the end of the second millennium.[78] But for our purposes it is not necessary to make any assumptions. If the people at Pithecusa were smelting iron in the eighth century, then the Euboeans and, no doubt, other Greeks had the required metallurgical skill by that date.

It follows that there was no need for eighth-century Euboeans to go and settle in the far West simply to obtain iron; there was plenty at home. The presence of iron in Tuscany does not, therefore, *in itself* explain why the colony was established.

There is also some inherent improbability in the notion that people used to working locally available iron sources should have welcomed the idea of bringing ironstone by ship from a place some considerable distance away, in order to smelt it at home.[79] We are

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[74] In addition to the works in the preceding note see *BSA* lxi (1966) 109-10.

[75] Pleiner 11-12, 19-20, 23-4; Forbes 457-8; Neumann and Partsch 236. It is not always easy to tell whether Greeks smelted their own iron or imported iron in semi-finished condition; so much so that Forbes actually states that they mostly imported it and on the same page (458) discusses local smelting in Greece. No doubt economic considerations decided the matter differently in different times and different places.

[76] Pleiner 11-12; Snodgrass, *AJA* lxvi (1962) 408-10.

[77] See Pleiner *ibid.*

[78] *Ibid.*

[79] Some have thought that Strabo in the passage v 223 states that in his day the

assured that the iron smelted on Ischia did indeed come from Elba and that there is definitely no workable iron ore on Ischia.[80] It is necessary to remember, however, that quite a small || isolated seam (as of pyrite) would be sufficient for the requirements of the small-scale local iron industry of an ancient city, and such a seam could have been entirely worked out long ago. I mention this possibility only because there seem to me inherent general objections to the notion that the iron industry of eighth-century Pithecusa depended on sources of iron as distant as Elba.

There are also difficulties regarding the suggestion that Pithecusa was an emporion which lived by passing on the products of the East to Italian and particularly Etruscan peoples. It has been acutely pointed out that the abundance and diversity of products of eastern origin which we find at eighth-century Pithecusa is not in fact paralleled on any known contemporary site in Etruria.[81] The picture seems to be that Pithecusa itself must often have been the end of the trade route from the East, and the Pithecusans were largely acquiring these products for themselves. It has been suggested, therefore, that the trade between the Greeks and Etruscans at this time was mainly in primary products, and does not leave, in consequence, traces in the archaeological record.[82] But one is bound to ask, what primary products were the Greeks offering the Etruscans?[83]

These considerations suggest that it is still a complicated and difficult question why the first Greek colony in the West was the most distant. We can be sure that the people who settled there did

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ore was shipped from Elba to the adjacent mainland for smelting, but that does not seem to be the right interpretation; see the Loeb edition p. 354 n. 3. It was already smelted iron from Elba which was shipped at about the same period to the famous iron workings at Puteoli; see Diod. v 13.1-2. But I doubt if these references to the practices of a far later age are really significant for the period that we are discussing.

[80] See n. 69 above. I am grateful to Professor W. S. MacKenzie, Professor of Petrology, University of Manchester, for kindly discussing these geological questions with me and giving me the benefit of his personal knowledge of Ischia.

[81] Ridgway, *Dialoghi di Archeologia* i-ii (1969) 27-30.

[82] *Ibid.*

[83] It has been suggested to me that the Greeks at Pithecusa might have had the metallurgical skill to manufacture iron when it was still unknown to the Etruscans. This seems to be just conceivably possible, since it is thought that the knowledge how to produce iron reached Tuscany 'possibly by 800 BC' (Coghlan 71), but I should be hesitant to advance the paradoxical hypothesis that the Greek colonists brought their knowledge of iron-working to the West in order to sell iron to the peoples who were later so famous for their riches in metals.

so because they were better able to win a livelihood on Ischia than at home. But the way they won that livelihood is still not entirely clear. They had products to give in return for their imports from the East, and one of those products was probably iron, iron which could no doubt be traded at a profit in the West among people less skilled (or totally unskilled) in metallurgy.[84] The choice of an island seems to suggest they had security in mind, though Ischia is rather too big to be seen as the typical offshore island site. The fertility of the soil was doubtless attractive to a people used to a predominantly agricultural economy. If that were all, however, there must have been powerful reasons to deter them from settling elsewhere in southern Italy or Sicily. Such powerful reasons might have been furnished by the strength and hostility of the existing inhabitants. That would be a perfectly legitimate hypothesis.[85] We may yet find evidence for extensive trade between the Pithecusans and peoples in Italy in the eighth century. As the evidence at present stands, however, it would, curiously enough, be much easier to understand their choice if there was iron as well as fertile soil on Ischia itself.

The discoveries on Ischia have also great significance for our understanding of early Euboean colonisation as a whole. In the first place we now know definitely that there were Euboean colonies in the West for some fifty years before the first colonies in Sicily. During those years ships were regularly plying the coast route from Greece to southern Italy. Recent discoveries at an important site at the modern Francavilla, near ancient Sybaris, show the sort of goods those ships may have brought. Of the four levels which have been distinguished the two earliest antedate the establishment of Greek colonies in the area. Corinthian Geometric pottery was found in a tomb of the earliest period, which has been dated *c.* 800-750; and in the second phase, built directly on top of the first, there are imports from the East, scarabs, glass and amber.[86] ||

[84] As, for example, at the recently discovered site of Francavilla; see *AR for 1969-70* 45 and below, next paragraph. Trading in iron is attested by Homer; see, e.g., *Od.* i 184.

[85] *Cf.* Dunbabin 43-7 on the use of force by Greek colonists against Sicels in Sicily and southern Italy.

[86] *AR for 1969-70* 45. The site is discussed by P. Bicknell, *Klearchos* xxxv (1967) 131-43, where an interesting argument is put forward in favour of its identification as Aminaia.

There is also no doubt that the Ionian islands will have been essential ports of call for those ships. Traces of early Euboean activity in this area were seen long ago in some dubious mythological evidence, and, more recently, signs of contact with Euboea have been recognised in the eighth-century pottery from Ithaca,[87] but the only explicit evidence that we have relates to Corcyra. Plutarch states, in a most suggestive passage,[88] that the Corinthian colonists expelled Eretrian settlers from Corcyra. The Corinthian colony at Corcyra was established either in 733 or 709,[89] so an Eretrian colony had been established there before one of those dates, if Plutarch may be trusted. Not everyone has been inclined to trust Plutarch on this matter,[90] but it is obvious that in general terms the results of the excavations on Ischia make it very probable that his information is reliable. In the light of what we now know of the Euboean colonisation in the West eighth-century Euboean settlements in the Ionian islands make very good sense.

Plutarch's statement does not only concern Eretrian colonisation in the West. He relates that the ejected Eretrian colonists tried to return to the mother city, were refused permission to land, and so sailed away to found a new colony at Methone. If we accept his information about Corcyra, we should also reconsider the implications of his statement regarding Eretrian colonisation in the Chalcidice region.

This is an area of colonial settlement where there are no literary foundation dates before the mid-seventh century, and also an area where none of the colonial sites has been excavated.[91] Arguments about the date of the colonisation of the region are, therefore, inevitably inconclusive. It is possible to interpret Plutarch's information about Methone as implying that Chalcidice was an area of extensive colonial settlement in the eighth century. Since Methone is not so attractive a site as Mende, the main Eretrian colony in Chalcidice (not to mention Torone, the greatest of the Chalcidian colonies), it seems likely that the best sites in the region had already

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[87] Wilamowitz, *Phil. Untersuch.* vii (1884) 171-2; on Ithaca see Coldstream 366-7.
[88] *QG* xi.
[89] *Cf.* my discussion, *Colony and mother city* 219-20; Coldstream 367.
[90] See Cook 71.
[91] For accounts of the colonisation of this area see Bérard 66-8, 92-5; Boardman 236-40. There is also a useful discussion in Bradeen's paper, *AJP* lxxiii (1952) 356-80.

been occupied when Methone was colonised. Such an interpretation would harmonise well with Aristotle's implication that there were Chalcidian colonies in 'Thrace' before the Lelantine War,[92] for the date of that war, in spite of its notorious uncertainty, seems best situated about the end of the eighth century.[93] In addition to these rather slender arguments we now have not only our improved knowledge of Euboean colonisation in the West but also the recent interesting discoveries about Eretria itself.[94] New Eretria is now known to have been laid out at about the same period as Pithecusa was established. It is suggested that it was created by the synoecism of several previous settlements, and it has been acutely called the first Eretrian colony.[95] The excavators have drawn attention to the grand scale of the new city, with its large space between the acropolis and the sea.[96] Thus we can now say that before the middle of the eighth century the Eretrians were trading widely in the Mediterranean; they had successfully settled colonies in the West; and they had synoecised at home, laying out a new city on a large scale. It is not difficult to imagine that these people were capable of colonisation in more than one area at that time.

In collecting arguments in favour of the idea that Chalcidice was colonised in the || eighth century we should also not forget that it has the great advantage of proximity and it is geographically a natural Greek colonial area.

However, it is also possible in the present state of our knowledge to mount quite a strong counter-argument. When R. M. Cook discussed the matter,[97] he argued that since Thasos is a particularly attractive site and since we know that Thasos was not colonised till the first half of the seventh century, it may be assumed that the other colonies are later in date. Thus he rejected Plutarch's evidence about Methone, and, incidentally, ignored the passage Arist. *Erotikos fr.* 3. But it is not necessary to treat the evidence so cavalierly. It is

92 *Erotikos fr.* 3 (*OCT*, Ross = *fr.* 98 Teubner, Rose).

93 For my views on this matter see *Colony and mother city* 222 n. 3.

94 For brief accounts of the recent excavations at Eretria see *AR for 1966-7* 12-13; *1968-9* 8; *1969-70* 7-8. Apart from the preliminary reports in *Antike Kunst*, some volumes of the final publication have already appeared (most recently *Eretria* iii. *L'heroon à la porte de l'ouest*, by C. Bérard [Berne 1970]).

95 Schefold, *ADelt* xxii (1967) *Chr.* i, 271; *cf. AR for 1968-9* 8.

96 Schefold, *Antike Kunst* ix (1966) 106ff., especially 108; xii (1969) 72-4; C. Bérard, *Eretria* iii 68.

97 71.

possible to accept what Plutarch says and still maintain that Chalcidice was at least largely uncolonised in the eighth century. The choice of Methone could be explained by the hostility of native peoples further east rather than by the assumption that the best sites in Chalcidice were already occupied. We know that Thasos was only settled in face of the warlike hostility of the local Thracian tribes.[98] If we envisage that the whole north Aegean area was in the eighth century made unattractive to Greek colonists by native opposition, we could see Methone as an isolated eighth-century venture, and follow Cook in taking Thasos for the first major Greek colony in the area. Aristotle's statement is too undefined to provide by itself a major obstacle to such an interpretation.

It will be seen that we are still in the position of being unable to answer definitely very simple questions about the early colonisation of Chalcidice. We cannot estimate the extent of the area denied to Greek colonists by Thracian tribes, and hence we cannot tell how much colonisation was achieved before the middle of the seventh century. Of the two views outlined above I prefer the former. It seems to me most probable that Chalcidice was an important area of Greek colonisation in the eighth century. But I am also well aware that the argument will remain inconclusive until we are provided with some new and definite evidence by the spade.

Many of the individual points and suggestions made here may be open to disagreement, but they should serve to illustrate some important conclusions. In the first place it is clear that geographical factors do not in themselves determine the pattern of Greek colonisation, even in the eighth century. So far as it depended on seafaring skill or geographical knowledge, the Greeks were able to colonise anywhere between the Bay of Naples and the Bosporus, if not beyond. The determining factors were political and economic. We are thus bound to try—difficult though it may be—to make reasonable estimates of the power and attitudes of existing inhabitants of colonial areas, and of the economic possibilities for the colonists, if we are to understand the pattern of colonisation. These factors have normally been given less attention in discussions of Greek colonisation than the economic and political conditions of the founding states.[99]

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[98] See above n. 28.

[99] As, for example, in the fundamental paper of A. Gwynn, *JHS* xxxviii (1918) 88-123.

But while these conditions undoubtedly produced the need for colonisation, the pattern which the colonisation took was largely determined by local factors in the colonial areas.

It is also to be hoped that this paper will have drawn attention to the ways in which our knowledge of Greek history in the early period is enriched and improved by the study of the colonising movement; to the advances that have been made in the study of that movement owing to recent archaeological discoveries; and, finally, to the very great scope that exists to make further improvements in our knowledge of early Greek colonisation by suitably directed archaeological exploration. ||

2. PRE-COLONIAL CONTACTS: QUESTIONS AND PROBLEMS

It is obvious that any serious colonization requires previous knowledge, both of the land to be settled and of the people who inhabit it. So there is a good theoretical case for the assumption that pre-colonial contacts between Greeks and native peoples were an essential feature of the Greek colonizing movement. Actual evidence for such contacts in the Archaic period, however, barely exists in our literary sources. It is not surprising, therefore, that any signs of pre-colonial contacts in the archaeological record evoke an enthusiastic response. Nevertheless, it is important to bear in mind the kind of information required to demonstrate pre-colonial contacts which are not only firmly established but also historically significant. This point can be illustrated by considering the best attested instance of such relations in the literary record, though, ironically, they did not lead to actual colonization.

In his description of the distant voyages of the Phocaeans, Herodotus tells us that they became so friendly with Arganthonius, King of Tartessus, that he tried to persuade them to leave their homeland and come and settle in his territory. When he could not so persuade them, he gave them large sums of money to build a defensive wall against the threat from Persia.[1] We may have confidence in the historicity of these facts. The approximate chronology is established by the mention of the rising power of Persia, which shows that Arganthonius' relations with Phocaea persisted till about the middle of the 6th century BC. Herodotus' information about that period is not likely to be seriously astray.

Herodotus also tells us that the Phocaean traders sailed in warships (penteconters) rather than merchant ships. The implication that the Phocaeans could be involved in fighting as well as trade has always been recognized, but the use of penteconters, with their numerous crew and small carrying capacity, also shows that the commerce must have been chiefly confined to goods of small bulk and high

[1] Hdt. 1. 163.

value. Although the exact position of Tartessus is still unknown, there seems to be little || doubt that it lay in the lower valley of the Baetis (Guadalquivir) in southwestern Spain, where there were abundant sources of metals, and notably silver.[2] Such goods suit well the character of the trade that the vessels imply. What they carried on the outward journeys is not known, but luxury products of some kind seem to be a necessary hypothesis.[3]

The essential elements for a good instance of pre-colonial contacts are all present in Herodotus' account. First of all, clear chronology. We know that these contacts occurred during Arganthonius' reign, which ended shortly before the Persian conquest of Asia Minor. If colonization had occurred, the contacts described would have immediately preceded it. The last is an important point. We need to know that these contacts took place not only before colonization, but also sufficiently close to it in time for there to be a real historical connection. The second essential point that Herodotus supplies is the identity of the visitors: they were Phocaeans. Thirdly, Herodotus makes clear the reason for their visit; it was trade, and we can confidently conjecture the goods that they sought. Finally, we know the character of their relations with the native people. They were very friendly, and conducted with the ruler himself.

Although we are not likely to achieve this degree of knowledge when we are dealing with archaeological evidence, we need to remember that all these elements are necessary for a proper understanding of pre-colonial relations.

These relations, where we can discover them, are an important part of the history of any individual act of foundation, but the most interesting cases are those which occur in an area before any Greek colonization has taken place. It is then that we can ask the significant general questions: how had the Greeks come to know the area, what

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[2] Strab. 3. 148. The great bulk of "the estimated 20 million tons of slag in the SW of the peninsula . . . that is clearly the result of smelting for silver" is pre-Roman and implies many centuries, or even millennia, of working; see J. C. Allan, *Consideration on the Antiquity of Mining in the Iberian Peninsula*, Royal Anthropological Institute Occasional Paper No. 27 (1970), esp. pp. 8-9, 17, 23.

[3] According to B. B. Shefton, *Die 'rhodischen' Bronzekannen*, Marburger Studien zur Vor- und Frühgeschichte 2 (1979), esp. p. 22, these goods would have included the famous so-called Rhodian bronze oinochoai. From the last third of the 7th cent. BC they are found over a large area, which seems to tally well with Herodotus' account of Phocaean voyages, and so Shefton hypothesizes that the Phocaeans were their distributors.

suggested to them that colonization would be possible, and what pre-colonial relationships had they formed with the native inhabitants? There are three areas where the evidence available allows profitable discussion of such pre-colonial relations: Sicily and South Italy, the Pontus, and the delta of the Rhône.

For the first of these regions we actually possess a direct statement in our literary sources about pre-colonial contacts. Ephorus says that, before the foundation of Naxus and Megara, Greeks were too frightened by the bands of Etruscan pirates and the savagery of the native peoples even to || sail to Sicily for trade.[4] However, his statement forms part of an account of the early Greek colonization in Sicily which can be shown to be false in many respects.[5] Furthermore, this particular assertion seems to be in direct contradiction to the view that Cumae was already founded before the first Greek colonization in Sicily, a view almost certainly held by Ephorus himself.[6] So we cannot accept this denial of pre-colonial trade, and must examine the archaeological evidence without any direct assistance from literary sources.

The earliest precisely dated colony in Sicily and South Italy is Naxus, founded in 734 BC according to Thucydides.[7] That date falls within the quarter century allotted to the period Late Geometric (LG) I in the accepted chronology of Greek pottery.[8] Except possibly at Pithecusa, no Greek material earlier than LG I has in fact been found at any Greek colony in Sicily or South Italy,[9] so the appearance in the area of Greek objects of earlier date constitutes prima-facie evidence of pre-colonial trade.

The material from the East, which is thought to attest pre-colonial trade, consists not only of Greek pottery of the Middle Geometric (MG) II period, but also of oriental objects, Egyptian and Egyptianizing scarabs, seals, faience pendants, plaques and statuettes, glass and amber.[10] The finest object is a bronze bowl, unearthed at Francavilla

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[4] *F.Gr.Hist.* 70 F 137.

[5] See on this passage J. Bérard, *La Colonisation grecque de l'Italie mériodionale et de la Sicile dans l'antiquité* (1957), 75-9.

[6] See *F.Gr.Hist.*, commentary on 70 F 134; K. J. Beloch, *Mem.Acc.Linc.* 10 (1881-2), 436-7.

[7] Thuc. 6. 3. 1; cp. T. J. Dunbabin, *The Western Greeks* (1948), 435ff. (appendix 1).

[8] See J. N. Coldstream, *Greek Geometric Pottery* (1968), 302ff.

[9] See R. Ross Holloway, *Italy and the Aegean, 3000-700 BC* (1981), 133.

[10] See e.g. the finds from Francavilla Marittima: P. Zancani Montuoro, *Atti M.*

Marittima, of Phoenician manufacture.[11] It is generally assumed that the Egyptianizing scarabs and other small objects were also made by Phoenicians, and that the actual Egyptian objects came out Egypt as part of the long-established commerce between that country and Phoenicia.[12] None of this oriental material is itself closely datable, so it only || possesses significance for our immediate purposes if it occurs in association with better dated objects. Such conditions obtain at several Italian sites, as, for instance, at Veii, pre-Hellenic Cumae, and Francavilla Marittima.[13]

Greek MG II pottery found at Italian and Sicilian sites mainly consists of chevron skyphoi (which are the most numerous of the finds to date), skyphoi with pendent semicircles (PSC), and 'protokotylai'.[14] Since the period to which these three types are assigned is dated, in round terms, 800-750 BC,[15] these vessels are attributed to a time earlier than all the Greek colonies in the West which have definite foundation dates. In addition, their relatively precise dating enables them to be used to give absolute dates to the imported oriental material with which they are associated. They therefore are the key to the concept of pre-colonial trade in Italy and Sicily, and their dating is of cardinal importance.

Grecia 11-12 (1970-1), 9-36; 15-17 (1974-6), 9-82. For the seals of the so-called lyre-player group see G. Buchner and J. Boardman, *J.d.I.* 81 (1966), 1-62.

[11] P. Zancani Montuoro, *Atti M. Grecia* 11-12 (1970-1), 9-36. For such Phoenician bowls in general, see H. Frankfort, *The Art and Architecture of the Ancient Orient* (1954), 195-201.

[12] See for the Egyptianizing character of Phoenician art in this period Frankfort, *Art and Architecutre*, 188-201; for the physical objects of a contemporary settlement in Phoenicia itself J. D. Pritchard, *Recovering Sarepta, a Phoenician City* (1978), 71-95. Although only a large trial in character, Bikai's excavation at Tyre is the sole scientific excavation to date in any of the main Phoenician cities of the homeland: P. M. Bikai, *The Pottery of Tyre* (1978). Cp. W. Röllig in: H. G. Niemeyer (ed.), *Phönizier im Westen*, MB 8 (1982), 15-23, who points out that vigorous commercial contacts between the Phoenician cities and Egypt are well attested in documentary sources from the Late Bronze Age down to Assyrian times, and that they seem to have been little interrupted, if at all, by the disturbances of the end of the Bronze Age.

[13] See Coldstream, *Greek Geometric Pottery*, pp. 355f.; Zancani Montuoro, *Atti M. Grecia* 11-12, p. 28 [For Veii see now J. Toms, *Ann.A.Stor.Ant.* 8 (1986), 41-97; for Cumae: B. d'Agostino in: M. Pallottino *et al.* (eds.), *Popoli e civiltà dell'Italia antica*, ii (1974), 29ff.; C. Albore Livadie in: *Napoli antica* (1985), 62ff. Ed.]

[14] See for illustrated examples of these types Coldstream, *Greek Geometric Pottery*, pls. 17h; 18d, e; 32e, g, h.

[15] Coldstream, *Greek Geometric Pottery*, pp. 310-13, 330f.

The absolute dating of Middle Geometric is a difficult and uncertain business,[16] and the historian who uses this material for his chronology should be aware that his absolute dates are subject to an even larger approximation than the usual quarter-century of play in all pottery dating. The main problem concerning the simple and popular cups under discussion is the length of time they were being manufactured. It has been noted quite recently that the chevron skyphoi were once 'objectively' dated to the end of the 8th century BC,[17] and there are certainly examples from some parts of the Greek world dated to the LG period.[18] The situation is even worse in the case of the PSCs. Their dating has been set as widely as *c.* 900-700 BC,[19] and several have been found in archaeological contexts which unquestionably belong to the late 8th or even early 7th centuries BC.[20] To place any given chevron skyphos and, || even more, any PSC in the MG II period requires, therefore, a confident decision about the typology, chronology, and origin of the vase.[21] Yet we know that none of these matters is easy to determine, especially when we are dealing with such simple and widely manufactured products.

The current orthodoxy among archaeologists concerned with 8th-century BC Italy, whereby the chevron skyphoi are placed in the second quarter of the century, and the skyphoi with pendent semicircles a little earlier,[22] is, therefore, not only ambitiously exact chronologically, but also based on uncertain foundations.

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[16] *Ibid.*

[17] M. Torelli in: *Insediamenti coloniali greci in Sicilia nell'VIII e VII secolo a.C., Cronache di Archeologia* 17 (1978), 147.

[18] For a full discussion of these skyphoi see Descœudres in: J.-P. Descœudres and R. A. Kearsley, *BSA* 78 (1983), 11-34, who concludes that, although most chevron skyphoi belong to the MG II period, some continued to be made all through the 8th and even into the 7th century BC.

[19] See R. A. Kearsley in: Descœudres and Kearsley, *BSA* 78, pp. 41-52 (add to the bibliographical survey p. 43: H.-G. Buchholz, *Berl.Jb.V.Früh.Gesch.* 5 (1965), 224-31).

[20] Dates in the later 8th or even early 7th century BC have been proposed for examples from Tarsus, Sardis, Salamis in Cyprus, Troy Settlement VIII, Larisa, Emporio (Chios), Thasos (for refs. see Kearsley in *BSA* 78, and Buchholz, *Berl.Jb. V.Früh.Gesch.* 5).

[21] Compare G. Voza's argument about the PSC skyphos from Villasmundo in: *Insediamenti coloniali greci*, p. 107, which comes to an entirely different conclusion from that of Kearsley, *BSA* 78, p. 52.

[22] See e.g. Voza, *loc. cit.*; J. de La Genière, *RA* 1978, 264. Note that Kearsley assigns all three PSC skyphoi found in the West to her Type 6, dating from the 2nd half of the 8th century BC.

We also have a more general chronological problem to consider. While Naxus is the first Greek colony in the West with a definite foundation date (and one well confirmed, in general terms, by archaeological discoveries),[23] it is very generally believed that Pithecusa, and possibly Cumae were founded earlier. If we ignore the obviously erroneous Eusebian date for Cumae of 1050 BC, we are left with only relative indications of the dates of these two colonies in our literary sources. Strabo says, in a passage most likely derived (directly or indirectly) from Ephorus, that Cumae is the oldest of the Greek cities in Italy and Sicily, and, according to Livy, the Chalcidians first settled at Pithecusa and then transferred to Cumae.[24]

Archaeological evidence found to date does not support Strabo. The earliest material found at Hellenic Cumae is of LG II date, *c.* 725-700,[25] whereas LG I material has been found at several Sicilian colonies in addition to Pithecusa. The best argument for an early foundation date for Cumae is indirect: LG I material has been discovered at Zancle which, according to Thucydides, was first settled from Cumae.[26] If we employ that argument, Cumae was founded *c.* 750-725 or earlier.

At Pithecusa, LG I material has been found in abundance, and G. Buchner has argued that the great size of the city in the LG period suggests that its actual foundation occurred earlier, and that evidence of this earlier occupation has not yet been found.[27] This argument has recently received what may be material support in the nine fragments of II chevron skyphoi from the unstratified dump, the 'Scarico Gosetti', which are claimed to be 'basically MG'.[28]

Thus some doubt attaches to the foundation dates of both Pithecusa

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[23] See *CAH* iii. 3 (2nd ed.), 103f. [For a recent discussion of the archaeological material see P. Pelagatti in: *Atti Conv. Atene* 1979, i. (1983), 291-311. Ed.]

[24] Strab. 5. 243; Livy 8. 22, 5-6.

[25] Coldstream, *Greek Geometric Pottery*, p. 326; W. Johannowsky in: *Contribution à l'étude de la société et de la colonisation eubéennes*, Cahiers du Centre Jean Bérard 2 (1975), 102.

[26] Thuc. 6. 4. 5. LG I material similar to that found at Naxus, Syracuse, Megara Hyblaea, and Leontini has been found at Zancle (Messina): see G. Bacci in: *Insediamenti coloniali greci*, pp. 100-3.

[27] In: *Contribution à l'étude de la société et de la colonisation eubénnes*, pp. 66f.

[28] See D. Ridgway in: *Nouvelle contribution à l'étude de la société et de la colonisation eubéennes*, Cahiers du Centre Jean Bérard 6 (1981), 45-60; id., *A. Rep. London* 1981-2, 66. [See now most conveniently id., *L'alba della Magna Grecia* (1984), 98ff. Ed.] However, there is room for doubt about the classification of these sherds as MG: see above, n. 18.

and Cumae. If one ignores the exiguous material thought to be MG at Pithecusa and Buchner's argument, both colonies could have been founded no earlier than the LG I period. In that case, imported material in the West of MG II date could be taken to be pre-colonial in the full sense. If, on the other hand, one accepts that Pithecusa was founded in the MG II period, there was at least one Greek colony in existence in the West at the time of the trade often described as pre-colonial. So chronological uncertainties make it unclear whether the trade in question was pre-colonial or protocolonial, and they are not the only problem.

Of the many basic questions about ancient trade, which are difficult to answer, 'who carried the goods?' is one of the most intractable. Even when we have a good wreck—normally our best evidence—it may be impossible to determine whose was the ship and who were the crew. Those who fall into the seductive temptation of assuming that, if the goods are Attic, the carriers were Athenians, if the goods Etruscan, the ship Etruscan, are guilty of an over-simplification, which may save thinking about a complex situation, but does not help us to discover the truth abou the past. We have solid proof from 'trade marks' that Greek fine pottery was often made in one state and carried by traders from another,[29] and we have abundant testimony that Phoenicians were carrying Greek pottery by the 8th century BC at the latest.

No wreck has yet been discovered to help us determine who carried the goods from the Aegean and the East which came to Italy in the 8th century BC, so the best we can do is weigh probabilities. It is clear on general grounds that there are two contenders, Phoenicians and Greeks. The Phoenicians were well established in the western Mediterranean, as colonists or traders or both, by the first half of the 8th century BC.[30] In Sicily, according to Thucydides, the Phoenicians had settlements on headlands and offshore islands all round the island, before the Greeks came, for the purpose of trade with the local people.[31] Although this statement has not been confirmed by archaeological discoveries,[32] it shows that there was a tradition that

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[29] See A. W. Johnston, *Trademarks on Greek Vases* (1979), 51f.

[30] See H. G. Niemeyer's recent and thoroughly documented survey "Die Phönizier und die Mittelmeerwelt im Zeitalter Homers," *Jb.Z.Mus.Mainz* 31 (1984), 1-94.

[31] Thuc. 6. 2. 6.

[32] The earliest material from Motya dates from the late 8th cent. BC: see B. S. J. Isserlin *et al.*, *N.Sc.* 24 (1970), 580-2.

Phoenicians preceded Greeks in ll contacts with Sicily. Furthermore, when Pithecusa was in existence and receiving eastern goods in quantity, we know that some Phoenicians visited, and some resided in, the Greek colony.[33]

So the Phoenicians were present at the right time and in the right place. As to the goods, we have seen that some of the eastern products found in Italy were made by Phoenicians, and those from Egypt probably came via Phoenicia. The earliest piece of Greek pottery found in Spain, which is definitely of MG date, was very probably taken there by Phoenicians,[34] and, in the second half of the century, they were bringing oil from Attica to Spain in the so-called SOS amphorae, which are also so common at Pithecusa and other Greek sites in Italy and Sicily.[35]

As to the Greeks, it has been widely assumed that the Euboeans were very active in this trade, or even that they controlled it,[36] and there are some obvious arguments in favour of such a hypothesis. Chalcis and Eretria were great cities in the 8th century BC, in contact with many areas overseas, including the eastern Mediterranean.[37] Since they founded Pithecusa, Cumae, Naxus, and other early colonies in Sicily, it is an easy step to the conclusion that it was they who carried the goods from the Near East and Greece to Italy. But what especially recommended that conclusion were the quantities of Levantine goods found at the Euboean colony of Pithecusa, in combination with the belief that the contemporary settlement at Al Mina was a Greek colony, in which Euboeans played a major role. Since this latter belief seems unfounded,[38] and since we know of direct Phoenician contact with Pithecusa, this particular combination is no longer persuasive. So, while a strong case can be made for the

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[33] See G. Buchner, *PP* 23 (1978), 130-42; id. in: Niemeyer (ed.), *Phönizier im Westen*, pp. 277-308. [See now also Ridgway, *Nouvelle contribution*, pp. 125ff. Ed.]

[34] See B. B. Shefton in: Niemeyer (ed.), *Phönizier im Westen*, pp. 343-6.

[35] See A. W. Johnston and R. E. Jones, *BSA* 73 (1978), 103-41; Shefton, *Die 'rhodischen' Bronzekannen*, pp. 338-42. However, Coldstream's comments (*Greek Geometric Pottery*, p. 369) show the improbability of Shefton's suggestion that the Phoenicians obtained the oil in the Italian area.

[36] See e.g. D. Ridgway in: C. Hawkes and S. Hawkes (eds.), *Greeks, Celts and Romans* (1973), 1-38.

[37] See e.g. J. N. Coldstream in: Niemeyer (ed.), *Phönizier im Westen*, pp. 261-72; V. Karageorghis and L. Kahil, *Ant.K.* 10 (1967), 133-5.

[38] For a brief expression of my opinion on this subject see *CAH* iii/3 (2nd ed.), 93; for a fuller treatment, *Dialogues d'histoire ancienne* 12 (1986), 51-65. [Cp. also J.-P. Descœudres in: *Eretria: Fouilles et Recherches*, vi (1978), esp. pp. 12, 15ff. Ed.]

Phoenicians as carriers of the eastern material which reached Italy in the 8th century BC, the arguments in favour of the Greeks, whether Euboeans or others, are less compelling.

As for the character of the relations with the native inhabitants, we can only make vague generalizations. Exchanges took place; foreign ships and their crews were familiar. Oddly enough, it is again the Phoenicians about whom we may conjecture more confidently. Thucydides' statement || about the Phoenicians in Sicily seems to imply close relations, such as we know existed later between the Phoenicians and the local inhabitants of western Sicily, the Elymians, from the end of the 8th century BC. There, early Motya was unfortified, and its survival depended on friendly relations with its native neighbours.[39]

In our second area, the Pontus, the evidence for early colonization is so poor that we are even uncertain in what century Greek colonization began. It is not in doubt that the majority of the colonies were established in the 7th and 6th centuries BC, but there are two on the south coast, Sinope and Trapezus, for which 8th-century BC dates are stated or implied in our literary authorities.[40] Trapezus has never been the scene of any significant excavation, but small digs were carried out at Sinope, both in the city and in a cemetery outside. The earliest datable pottery discovered was a sherd of Middle Wild Goat ware, which may be dated to the last third of the 7th century BC.[41] This and other pottery datable to the end of the century have been taken to confirm the low foundation date of 631 BC given by Eusebius.[42]

By itself the archaeological evidence from Sinope is insufficient for such a conclusion. The excavations have been too sporadic to create any confidence that the earliest material is known.[43] However, a low date for the foundation of Sinope was seen as consistent with

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[39] *CAH* iii/3 (2nd ed.), 186.

[40] *CAH* iii/3 (2nd ed.), 123.

[41] For the excavations at Sinope see E. Akurgal and L. Budde, *Vorläufiger Bericht über die Ausgrabungen in Sinope* (1956), and Y. Boysal, *AA* 1959, 8-19. Boysal dates everything from the pit in section B to the second quarter of the 6th century BC because it included an Attic black-figured sherd of that date. I owe the identification of the sherd of MWG II ware to J. F. G. Hind, "The Greek Colonization of the Black Sea Area in the Archaic and Classical Periods" (unpubl. Cambridge Ph.D. thesis, 1971), 'South Coast', p. 26.

[42] See e.g. Akurgal and Budde, *Vorläufiger Bericht*; Boysal, *AA* 1959; cp. J. Boardman, *The Greeks Overseas* (3rd ed., 1980), 240f.

[43] Cp. A. J. Graham, *BICS* 5 (1958), 32f.

the low dating of all Greek contacts with the Pontus, which was fashionable at the time the excavations in Sinope were carried out. Apart from general considerations, such as that the colonization in the Pontus should follow the establishment of Greek colonies on the Bosporus,[44] the low dating rested on two contentions: first, that no Greek material earlier than the late 7th century BC had been found in the Pontus;[45] and second, that the Greeks were not even capable of sailing up the Bosporus into the Pontus before they had invented fast rowing ships in *c.* 700.[46] The second of these contentions was patently false, and has been comprehensively refuted.[47] || As to the first, it can only be maintained by explaining away a small number of definitely earlier finds.

In 1909, the Imperial Archaeological Commission acquired a small Geometric hydria from dealers who said that it came from Berezan (Fig. 1).[48] It is called Attic or Atticizing by N. Coldstream who assigns it to MG II, i.e. to *c.* 800-760 BC.[49] Because its provenance depends on information from dealers, modern scholars have expressed doubts that it came from Berezan.[50] Against them we must set the fact that the archaeologists on the spot at the time accepted that the vase came from Berezan. The provenance is, strictly, uncertain, but, if the vase had not seemed chronologically embarrassing, I doubt if its finding place would ever have been questioned.

In 1939, C. A. Rodewald, then a student at the British School at Athens, visited the dig at Istrus, where M. F. Lambrino gave him a group of sherds that had been found in the excavation of the site.

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[44] The foundation dates for Chalcedon and Byzantium are 685 and 659 BC. However, since Herodotus (4. 144. 2) states that 17 years elapsed between the two foundations, either the higher date should be lowered, or the lower raised, by 9 years.

[45] See R. M. Cook, *JHS* 66 (1946), 82.

[46] R. Carpenter, *AJA* 52 (1948), 1-10.

[47] See B. W. Labaree, *AJA* 61 (1957), 29-33, and Graham, *BICS* 5, pp. 25-42.

[48] B. V. Farmakovsky, *AA* 1910, 227.

[49] *Greek Geometric Pottery*, p. 377 with n. 7 and table on p. 330.

[50] E.g. *ibid.*; P. Alexandrescu, *Histria*, iv. *La Céramique d'époque archaïque et classique* (1978), 21 n. 15; R. M. Cook, *JHS* 66 (1946), 76 n. 90, whose argument that most pottery from Berezan was broken is especially weak. There are plenty of complete pots from Berezan; see already E. H. Minns, *Scythians and Greeks* (1913), 416. For good accounts of Skadovsky's unpubl. records of his excavations of graves at Berezan early this century, see Hind (cit. n. 41), 'North Coast', pp. 37ff. and A. Kocybala, "Greek Colonization on the North Shore of the Black Sea in the Archaic Period" (unpubl. Ph.D. thesis, Univ. of Pennsylvania, 1978), 185, 193-202.

In 1950, Rodewald handed over these sherds to R. M. Cook, and they now form part of the collection of the Museum of Classical Archaeology at Cambridge.[51] The group contains the rim of a LG kotyle (Fig. 2), identified by Coldstream as a Euboean copy of a Corinthian type dated to *c.* 750-720 BC.[52]

Also found at Istrus are six small sherds which belong to one or more jugs of Cypriot 'White Painted IV' ware from the Cypro-Archaic I period.[53] On the basis of many contexts in which Cypro-Archaic I wares are associated with better dated Greek pottery, A. Demetriou has convincingly shown that the widest termini for the date of this phase are *c.* 740-660.[54] This finding invalidates the opinion of J. Boardman, according to whom, "one may not use the Cypriot sherds to raise the foundation date of Histria, but rather the other Greek pottery will date II the Cypriot sherds—i.e. late 7th c."[55] In fact, these few scraps present us with pottery which seems to be earlier, not only than the Greek pottery of the last quarter of the 7th century BC, which is commonly regarded as the earliest from the site, but also than the Eusebian foundation date of 657 BC. It is extremely interesting that similar Cypriot pottery is also reported to have been found on Berezan.[56]

One of the finest vases from Istrus is an amphora, which P. Alexandrescu has acutely seen to have similarities which so-called 'G 2-3' ware.[57] Although this ware remains somewhat enigmatic, its general dating to the first half of the 7th century BC, and possibly to the later part of the 8th, seems reasonably clear.[58]

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[51] These circumstances are briefly reported in the typed catalogue of the collection in the Museum of Classical Archaeology by R. M. Cook, who gave the same information in a letter to Alexandrescu, which is quoted in *Histria* iv. 21 n. 15. I have had the benefit of discussion with my former colleague at Manchester, Mr. C. A. Rodewald, who also kindly wrote me a full letter on the subject. Although the high date of this sherd may be thought chronologically embarrassing, I regard it as in the highest degree improbable that, if Rodewald said that it came from Istrus, it should in fact have come from somewhere else.

[52] *Greek Geometric Pottery*, p. 377 n. 8; and cp. p. 191.

[53] Alexandrescu, *Histria* iv. 63, no. 256, pl. 26 (note 'cinq frs.', but six are illustrated).

[54] *AA* 1978, 12-25, Cp. Karageorghis and Kahil, *art. cit.* (n. 37).

[55] As quoted by M. Cadogan in: Alexandrescu, *Histria* iv. 63, no. 256.

[56] See *ibid.*

[57] Alexandrescu, *Histria*, iv. 62f., no. 253, pl. 1.

[58] See P. Bernard, *BCH* (1964), 88-105; A. J. Graham, *BSA* 73 (1978), 67-9; J. N. Coldstream, *Geometric Greece* (1977), 263f.; M. B. Moore in: K. Lehmann (ed.), *Samothrace*, v (1981), 317-71.

These pots may be few in number, but they are sufficiently numerous to show that it is wrong to try to explain away any or all of them. They demonstrate that the famous oinochoe from Temir Gora near Panticapaeum, and the other similar oinochoai which have been found in the Pontic region, which are dated *c.* 650-625, are not the earliest Greek vases found in the Pontus,[59] and that there were at least sporadic contacts between the Aegean and the north and west shores of the Pontus from the 8th century BC. This accords well with the testimonies of Greek literature, which demonstrate, to quote from R. Drews's perceptive study, "beyond reasonable doubt that by 700 the Black Sea was already becoming familiar to the Greeks."[60] While this is important, it falls a long way short of a satisfactory picture of pre-colonial relations. Our chronology is neither clear nor certain. If there were early foundations on the south coast, as I prefer to believe, we are dealing with a protocolonial rather than a pre-colonial situation. We do not know who brought the early goods from the East Mediterranean into the Pontus, nor do we know what they sought, though plausible guesses can be made on the basis of Greek literature and the better known conditions of later times. As for the character of the relations between Greeks and natives, we know from literary sources that some of the local peoples were regarded as frightening savages,[61] but the existence of exchanges and the later colonization show that this cannot be the whole story. ||

The third area for consideration is the delta of the Rhône. The first necessity here is to establish when Greek colonization began. There are statements in the literary sources that the Phocaeans were preceded by the Rhodians, who founded Rhode in the Gulf of Rosas, before it became a Phocaean colony.[62] Since Strabo says that the Rhodian colonization took place many years before the establishment of the Olympic Games, we may have here an example of sup-

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59 See Boardman, *The Greeks Overseas*, pp. 243f.; P. Alexandrescu, *RA* 1975, 65; G. Sokolov, *Antichnoe Prichernomor'e* (1973), 27, fig. 7; R. M. Cook, *Greek Painted Pottery* (2nd ed., 1972), pl. 30A. Cp. also P. Alexandrescu in: *Les Céramiques de la Grèce de l'Est et leur diffusion en Occident*. Colloques Int. du CNRS 569, Naples 1976 (1978), 53; and id., *Histria* iv. 20.

60 *JHS* 96 (1976), 22. The older view that the crucial lines, Hom. *Il.* 2. 853-5, are a late interpolation, has been convincingly refuted by Drews, *JHS* 96, pp. 20f. and by S. M. Burstein, *Cl.Ph.* 71 (1976), 339-41.

61 Cp. e.g. Hdt. 4. 64-5, 103; Strab. 7. 298.

62 Strab. 3. 160; 14. 654; pseudo-Scymnus 202-9; cp. Pliny *HN* 3.33, who confuses Rhodanusia with Rhode.

posed Greek colonization in the 'heroic' period. In any case, it has long been recognized that the tradition of Rhodian colonization may have arisen merely from the local river- and place-names Rhodanus, Rhode, and Rhodanusia.[63] Taken by itself, therefore, the literary tradition of Rhodian colonization seems very suspect. However, it has been thought by some scholars to be supported by material finds. There are, in the first place, East Greek vases among the early imports into the region, which were until recently generally classified as 'Rhodian'.[64] Of the many objections to the use of this pottery as a support for the tradition of early Rhodian colonization, two are fatal. First, it is now recognized that the pottery once called Rhodian was in fact produced by several East Greek cities.[65] Second, there is a vast chronological divide between the Rhodian colonization of our literary sources, said to be long before 776 BC, and the pottery thought to attest Rhodian presence, which is of the late 7th and 6th centuries BC.[66]

A second material support for the Rhodian tradition has been seen in the bronze jugs called 'Rhodian oinochoai'.[67] These are found in large numbers in Italy, more sporadically over a large area of the West, and occasionally in the eastern Mediterranean.[68] One example was discovered in a tumulus tomb at Pertuis, in the valley of the Durance in Provence.[69] These oinochoai make their appearance in the last third of the 7th century BC,[70] and the example from Pertuis may be dated to that period.[71] Thus, this material too would be vulnerable to the chronological objection mentioned above. Furthermore, recent discussions make it unlikely that these jugs were either made or transported by Rhodians.[72] ||

The suspect literary tradition of Rhodian colonization in this area is thus without archaeological support. On present evidence, the

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[63] See F. Villard, *La Céramique grecque de Marseille* (1960), 73.

[64] See e.g. *ibid.*

[65] See J. M. Cook in: *CAH* iii/3 (2nd ed.), 205-11.

[66] See Villard, *La Céramique grecque*, p. 73.

[67] E.g. by Villard, *La Céramique grecque*, pp. 73f.

[68] See Shefton, *Die 'rhodischen' Bronzekannen*, pp. 6-9, for the geographical distribution of these vessels.

[69] Shefton, *Die 'rhodischen' Bronzekannen*, pp. 22, 66, no. A18; B. Bouloumié, *Gallia* 36 (1978), 219-40.

[70] See Shefton, *Die 'rhodischen' Bronzekannen*, pp. 16f.

[71] Bouloumié, *loc. cit.*

[72] Bouloumié thinks that the geographical distribution suggests that they were of Etruscan manufacture; for Shefton's view see above, n. 3.

Phocaean colony of Massalia may be taken to be the first Greek colony in this area, and Massalia's foundation date is now known to be certainly *c.* 600 BC.[73] So we may regard that date as the time before which we have a fully pre-colonial period in the delta of the Rhône.

The earliest imported Greek material so far discovered in the area seem to be the three cups (and possibly also the oinochoe) found in a cemetery of the First Iron Age at Agde,[74] and a cup from the Grand Bassin I cemetery at Mailhac.[75] All the cups are Corinthian in type, but were probably made in South Italy or Sicily.[76] They are dated to the third quarter of the 7th century BC.[77]

We have here, it seems, imported Greek pottery which is sufficiently early and sufficiently well dated to be called pre-colonial. It is not so straightforward a matter with the remaining Greek pottery commonly attributed to the pre-colonial period. There is East Greek and Corinthian pottery from several sites in the area, which is normally dated to the last third or last quarter of the 7th century BC.[78] Given the inevitable approximations of pottery dating, this material is perhaps too close to the foundation of Massalia to be termed pre-colo-

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[73] Although there is a high date, *c.* 600 BC, and a low date, *c.* 545 BC, in the literary tradition, it is now certain, from plentiful archaeological evidence, that the high date is correct; see Villard, *La Céramique grecque*, pp. 76-81; J.-P. Morel, *BCH* 99 (1975), 866f.

[74] See A. Nickels *et al.*, *MEFRA* 93 (1981), 90-125.

[75] See M. Louis *et al.*, *Le Premier Age du Fer Languedocien*, ii (1958), 62-4, 69, fig. 48, with the correction of Villard, *La Céramique grecque*, p. 74 n. 5. For the dating see Nickels *et al.*, *MEFRA* 93, p. 100. [See now also J.-J. Jully, *Céramiques grecques ou de type grec et autres céramiques en Languedoc méditerranéen, Roussillon et Catalogne*, ii/1 (1983), 480f., pl. A1. Ed.]

[76] Nickels *et al.*, *MEFRA* 93, p. 99. Villard, *La céramique grecque*, p. 74 n. 5, had classified the cup from Mailhac as an Etruscan imitation of Protocorinthian. [Two PC fragments from Sanilhac/Grotte Saint-Vérédème seem to be proper Corinthian, dating to the 3rd quarter of the 7th cent. BC: see Jully, *Céramiques grecques*, ii/1 1016, nos. 001-002. Ed.]

[77] Nickels *et al.*, *MEFRA* 93, pp. 99f. The chronology is best established by tomb groups, where cups of this kind are associated with other, well dated material; for such associations see e.g. C. Albore Livadie in: *Le* bucchero nero *étrusque et sa diffusion en Gaule Méridionale*, Collection Latomus 160 (1979), 91-110.

[78] E.g. East Greek bird bowl, rosette skyphos, and PC skyphos from Saint-Blaise: see H. Rolland, *Fouilles de Saint-Blaise (Bouches-du-Rhône)*, *Gallia*, Suppl. 3 (1951), 59-63 with figs. 91-8; id., *Fouilles de Saint-Blaise (1951-1956)*, *Gallia*, Suppl. 7 (1956), 51; id., *Gallia* 22 (1964), 569-72; id., *CRAI* (1963), 81-9. For a Corinthian olpe of the Transitional period, an Ionic cup of the A1 type, and an East Greek bird bowl from La Couronne see C. H. Lagrand, *Gallia* 17 (1959), 179-201. [For further sites with early material see Jully, *Céramiques grecques*, i. 349-50. Ed.]

nial on date alone. However, there is a rather better argument from stratigraphy. At many sites it is possible to recognize the levels which clearly postdate the foundation of Massalia, because they contain the characteristic products of the Phocaean colonists, such as grey ware with wavy line decoration, 'pseudo-Ionian' painted pottery, and Massaliot amphorae. The pottery || we are considering was found in earlier levels, which do not contain the typical products of Massalia.[79]

The Greek material mentioned far from exhausts the ceramic imports into the region in the pre-colonial period. It is eclipsed in quantity by Etruscan bucchero and Etruscan amphorae. The bucchero consists predominantly of the well-known kantharoi, but other shapes associated with wine-drinking were also imported.[80] Etruscan amphorae have been found at many sites in the delta of the Rhône, and at several of them they are either the earliest, or among the earliest, imported pottery found.[81]

We thus possess relatively rich evidence for at least part of the material imported by native communities in the delta of the Rhône before any Greek colonies were founded. The life of these communities is also relatively well known from many careful archaeological investigations of sites such as Cayla-de-Mailhac.[82] In the First Iron Age, *c.* 700-600 BC, these people lived on defensible hills and their

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[79] For examples of this production see F. Benoît, *Recherches sur l'hellénisation du midi de la Gaule* (1965), pl. 43, 47-50. For some of the sites which present this stratigraphic sequence see above n. 78 (for the stratigraphy at Saint-Blaise see the important observations by B. Bouloumié in: *Le* bucchero nero *étrusque et sa diffusion en Gaule Méridionale* (1979), 111).

[80] Cp. T. B. Rasmussen, *Bucchero Pottery from Southern Etruria* (1979), 104-6, pls. 31-2. Bucchero was found in impressive quantities at Saint-Blaise, and some of it is early: see Bouloumié, *loc. cit.*; it was also found in the earliest levels at La Couronne (Lagrand, *loc. cit.*) and at La Liquière (F. Py and M. Py, *MEFRA* 86 (1974), 145-7). The earliest piece of bucchero found in the whole area comes from Massalia: a ribbed fragment thought to come from a chalice and dated to "no later than the third quarter of the 7th century" by Rasmussen, *op. cit.*, pp. 151, 156 (for an illustration see Villard, *La Céramique grecque*, pl. 3. 10). Yet very little pre-600 BC material in total has been found at Marseilles, and no true pre-colonial occupation has been recognized: Villard, *op. cit.*, pp. 76f.

[81] See B. Bouloumié, *RevArch. Narbonnaise* 9 (1976), 23-43; Py and Py, *MEFRA* 86, pp. 141-254; M. Py in: M. Cristofani *et al.* (eds.), *Il commercio etrusco arcaico*, Quaderni del centro di studio per l'archeologia etrusco-italica 9 (1985), 84. The very low chronology advocated by Py seems to conflict with that of both Early Corinthian and the Ionian cups.

[82] For a good brief account of the excavations, with bibliography, see O. Taffanel *et al.*, *Gallia* 37 (1979), 1-3; see also J. Briard in: S. Pigott *et al.* (eds.), *France before the Romans* (1973), 154, and F. R. Hodson and R. M. Rowlett in the same work, p. 159.

material culture was relatively simple. The economic basis of life was chiefly agricultural: agrarian cultivation in the valley bottoms and coastal plains, pastoral farming on the higher land. In addition, there was hunting in the hills, fishing in the rivers and on the coast, and—a special resource—abundant supplies of salt in the many salt lakes of the numerous estuaries. Although we can only guess at the political arrangements of these people, the character of settlement and information from later times allow us to assume that political units were small and under monarchical rule.[83] ||

Among the goods they were eager to import were wine and fine pottery and metalware connected with wine-drinking. There are obviously two major questions about this trade. What goods were sought in return by the merchants from overseas? And, who were those merchants?

Earlier scholars had little doubt about the answer to the first of these questions. Metals—copper, tin, gold, and silver—were the primary attraction, with salt, salt-fish, and medicinal herbs as important subsidiaries.[84]

The herbs are uncertain,[85] but the salt and salt-fish look convincing. This was, and is, a great area for salt production, and a site like Saint-Blaise, where the evidence for imports is impressive in quantity and begins very early, dominates great lagoons and salt-beds. The metals seem problematic. The rivers that debouch in the Gulf of Lion, notably the Rhône itself, certainly offer routes to many known metal deposits,[86] but we only have evidence for the flow of goods from the Mediterranean along these routes after the foundation of Massalia. Even then it is doubtful if the famous tin

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[83] Compare e.g. the many names of tribes: see G. Barruol, *Les Peuples préromains du sud-est de la Gaule* (1969), 109-11; the chief of the Segobrigii in the story of Massalia's foundation (Justin 43.3.8; Arist. fr. 549 [Teubner]); and the many 'chief's tombs' that have been recognized (see e.g. O. Taffanel and J. Taffanel, *Gallia* 18 (1960), 1-37; B. Bouloumié, *Gallia* 36 (1978) 219-40).

[84] See e.g. Benoît, *Recherches sur l'hellénisation*, pp. 191-213 and for less bold, but still largely uncertain suggestions, J.-P. Morel in: *Atti XII Conv.St.Etr.Ital.* (1981), 488-90, and id. in: *Modes de contacts et processus de transformation dans les sociétés anciennes, Actes du colloque de Cortone 1981*, Collection de l'École Française de Rome 67 (1983), 554f.

[85] Benoît, *Recherches sur l'hellénisation*, pp. 191, 199 depends heavily on an implausibly close interpretation of Strabo's words (14. 654) *ἐπὶ σωτηρίᾳ τῶν ἀνθρώπων*, in a passage supposedly referring to many years before 776 BC!

[86] See Benoît, *Recherches sur l'hellénisation*, pp. 191-4; Morel in: *Atti XII Conv.St.Etr.Ital.* pp. 489f.

route from Cornwall via the Seine and Rhône to Massalia was in use.[87]

There are three peoples to consider as possible carriers of the pre-colonial imported goods: Etruscans, Phoenicians, and Greeks. The current majority opinion that the trade was either totally or mainly in the hands of the Etruscans goes back to the discovery of the famous wreck off the Cap d'Antibes. Dated *c.* 570-560, it contained at least 160 Etruscan amphorae in association with Etruscan bucchero and Italo-Corinthian skyphoi, which are thought to be Etruscan copies of Corinthian ware.[88] Regrettably, the wreck has not been fully excavated, but among the finds was a simple terracotta lamp, which must have been part of the equipment of the crew. It is Phoenician.[89]

A quantitative argument has the attraction of simplicity: Etruscan material predominates by far, so the carriers were Etruscans. However, || such an argument is merely another instance of the false assumption that goods of a certain origin will be carried by people of the same origin. The situation is certainly not so simple; there is no site in the delta of the Rhône where the imported material is exclusively Etruscan, and the newly described wreck off the Isola del Giglio contained plenty of Greek pottery in addition to its Etruscan amphorae.[90]

It would be naïve to imagine that all the Etruscan bucchero and other Etruscan products, which are found all over the Mediterranean and even in the Black Sea, were carried in Etruscan ships.[91] The existence in Etruria of communities of foreign merchants, such as those at Pyrgi and Gravisca, is enough to show that others were involved.[92] On the other hand, the temptation to go to the other

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[87] See A. J. Graham in: *CAH* iii/3 (2nd ed.), 140f.

[88] The wreck and the finds are fully discussed by C. Albore Livadie in: *Hommage à Fernand Benoît* I, *Revue Études Ligures* 23 (1967), 300-26. That the ship and its cargo were Etruscan was suggested by F. Benoît, *Gallia* 14 (1956), 33; cp. B. Bouloumié and B. Liou, *Rev.Arch. Narbonnaise* 9 (1976), 211. For a recent typology of Etruscan amphorae see M. Py in: Cristofani *et al.*, *Il commercio etrusco arcaico*, pp. 73-94.

[89] See Albore Livadie, *Hommage à F. Benoît*, i. 319f.; the lamp is illustrated in Benoît, *Recherches sur l'hellénisation*, pl. 41, no. 6.

[90] See M. Bound in: Cristofani *et al.* (eds.), *Il commercio etrusco arcaico*, pp. 65-70.

[91] *Pace* M. Gras in: *Mélanges offerts à Jacques Heurgon*, Collection de l'École Française de Rome 27 (1976), 341-70. For the distribution of exported Etruscan objects, see *ibid.* 370, fig. 1; Morel, *Atti XII Conv.St.Etr.Ital.*, figs. 1-2; Rasmussen, *Bucchero Pottery*, pl. 64.

[92] For Gravisca see *CAH* iii/3 (2nd ed.), 142; Morel, *Atti XII Conv.St.Etr.Ital.*, pp.

extreme, and assume that all Etruscan goods were carried by others, should be resisted, as our little literary evidence shows.

Aristotle alludes briefly to commercial agreements between Etruscans and Carthaginians,[93] and the first treaty between Rome and Carthage reported by Polybius shows that Romans were expected to trade in Sardinia, Sicily, and Africa.[94] This treaty may well reflect the conditions of regal Rome, under Etruscan kings, and is close in date to the time in which we are interested. So we should certainly regard Etruscan traders in the delta of the Rhône as a possibility.

As for the Phoenicians, their amphorae are known in the area in the 6th century BC, and at one site, Mont Garou, they were found in association with Etruscan amphorae in a level stratigraphically pre-colonial.[95] The Phoenician lamp may be a clue to the nationality of the ship wrecked off Cap d'Antibes. They were famous traders and it was their normal practice to traffic in goods made by others.

Of the Greeks, the Phocaeans are obviously the first candidates for consideration. They began to establish colonies in the area about a || generation, or a little more, after the first testimonies to overseas trade appear, and the romantic story of Massalia's foundation implies at least some pre-colonial trade by Phocaeans.[96] On the other hand, the material record seems to tell against Phocaean involvement. Unless we follow Shefton in thinking that the 'Rhodian oinochoai' were distributed by Phocaeans,[97] there is nothing in the pre-colonial material known which points to Phocaea, and, more strikingly, the later material, which we know to have been associated with Phocaeans at Massalia, is quite different. Nor do the pre-colonial goods, wine amphorae and pottery, suit well Herodotus' information that the

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862-3; M. Torelli, pp. 26 (1971), 44-67; 32 (1977), 398-458 [now also M. Cristofani, *Gli Etruschi del mare*, 'Archeologia' 6 (1983), 122-4; F. Boitani Visentini in: M. Bonghi Jovino (ed.), *Gli Etruschi di Tarquinia.* Exhibition catalogue, Milan (1986), 250-2 Ed.]. For Pyrgi see J. Heurgon, *JRS* 56 (1966), 1-15 [and, for short summaries and further bibliographies, Cristofani, *op. cit.*, pp. 119-22; G. Colonna (ed.), *Santuari d'Etruria.* Exhibition catalogue, Arezzo (1985), 127ff. On the question in general: M. Martelli in: M. Cristofani (ed.), *Civiltà degli Etruschi.* Exhibition catalogue, Florence (1985), 175ff. Ed.].

[93] Artist., *Pol.* iii. 1280[a] 35ff. For a penetrating discussion of this passage see P. Gauthier, *Symbola: Les Étrangers et la justice dans les cités grecques* (1972), 90-2, 102f.

[94] Polyb 3. 22. 5-6, 9; 23. 1-3, 5; cp. F. W. Walbank, *A Historical Commentary on Polybius* I (1957), *ad loc.*

[95] See F. Benoît, *Gallia* 18 (1960), 308-10.

[96] Justin 43. 3.4-12; Artist. fr. 549 [Teubner]; Plut., *Vit. Sol.* 2.

[97] See above, n. 3.

Figure 1. Leningrad, Ermitage. MG hydria stated to come from Berezan (after AA 1910, 227 fig.27)

Figure 2. Cambridge, Museum of Classical Archaeology IS.I Rim fragment of LG kotyle from Istrus. Photo courtesy Museum of Classical Archaeology

Phocaeans traded in penteconters. These arguments may not be decisive, but they surely make it unlikely that the Phocaeans were solely responsible for the pre-colonial trade. Obviously they and other Greeks may have had a share in it.

These considerations show that any one, or two, or all three of the Etruscans, Phoenicians, and Greeks could have been the carriers of the pre-colonial goods. In the 6th century BC, sites in the region imported Greek, Phoenician, and Etruscan amphorae. We seem to have a rather plural and complex situation, which should make us chary of hypothesizing any one trading nation to the exclusion of others.

In the delta of the Rhône the evidence for pre-colonial contacts is in many respects better than for other areas. We have a clearly defined chronology and a good picture of the trade. For a generation or more before the first Greek colony was established, there were regular trading contacts and peaceful relations between foreign merchants and many native communities. Uncertainty about the identity of the carriers makes our knowledge seriously incomplete, but we can exclude any simple pre-colonization hypothesis, by which the trade was in the hands of the later colonizers.

This investigation of the pre-colonial relations in three major areas has shown that we are still a long way from having achieved the kind of knowledge that a historian would find satisfactory. Often the chronology itself, the basis for the whole idea of a pre-colonial phase, is still in doubt. However, we have already made some progress in a subject of great historical importance, and we may confidently expect that our evidence will be increased by new archaeological discoveries. ||

3. COMMERCIAL INTERCHANGES BETWEEN GREEKS AND NATIVES

If we think of commerce between Greeks and peoples who were less advanced in material culture, what comes first to mind? Probably the splendid and valuable works of art, which have been found at considerable distances from Greece and from Greek colonies, such as the precious objects in gold and silver from Scythian tombs, the magnificent Greek metalwork from Trebenište, the so called 'Rhodian' oenochoae, or the enormous bronze crater of Vix.

The powerful impact made by such dramatic discoveries on our imaginations is, unfortunately, not matched by our knowledge and understanding. The Scythian finds are usually classified as Graeco-Scythian art,[1] and it is generally recognized that they were made by Greek craftsmen to satisfy Scythian tastes, but we simply do not know in what city, or part of the Greek world, they originated, and we are reduced to guessing possibilities, when we try to establish either by what routes they came, or whether they should be regarded as diplomatic gifts, tribute of some kind, or one part of a commercial exchange.[2]

Most of the Greek metalwork at Trebenište, including the splendid bronze vessels, was thought by Jacobsthal to be Peloponnesian, but the finds as a whole also include East Greek products.[3] Using the origin of the goods and a map, Jacobsthal assumed that most of the Greek material came to Trebenište from the West, via Epidamnus and Apollonia, which are about eighty miles distant, but that the East Greek goods came from the coast to the East, an overland journey of 140 miles. This argumentation seems in danger of

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[1] E.g., R. A. Higgins, *Greek and Roman Jewellery* (London 1961) 118; cf. 11, 27, 122; J. Boardman, *The Greeks Overseas*[3] (London 1980) 258-263.

[2] Boardman, for instance, assumes Ionian artists at work in the Pontic colonies; *op. cit.* 259, 264. For a significant similarity between Olbian and 'Scythian' goldwork technique in the sixth century, see Higgins, 11.

[3] P. Jacobsthal, *Greek Pins* (Oxford 1956) 201-208; followed by N. G. L. Hammond, *Epirus* (Oxford 1967) 436-439. For Trebenište in general, see Boardman, 236f., and, for more recent work, P. Lisiçar, 'Lychnidian lacus', *Parola del Passato* xxxv (1980) 7-14.

oversimplifying the complexities of sixth-century trade. The involvement of Corinth, which may have been the place of manufacture of some of the metalwork, and which had colonies at both the western and eastern ends of the land route in question, is a likely enough speculation, but the evidence does not allow us anything more than possible or probable hypotheses.

The elegant bronze jugs, known as 'Rhodian oenochoae', have been found in large numbers in Italy, less frequently over a very wide area of the West, and occasionally in the Eastern Mediterranean. In a recent study,[4] Shefton has divided the finds broadly into those of Greek manufacture and Etruscan copies. The former appear from the last third of the seventh century and persist into the sixth, while the Etruscan copies are apparently confined to the sixth century, especially its second quarter. Shefton thinks that the || Greek products originated in East Greece, and that their diffusion was the work of the Phocaeans, but the first contention rests on some fairly slight art-historical considerations, and is, in any case, rather vague, while the second arises from a comparison of the map of find-places with Herodotus' account of Phocaean voyages.[5]

Only at Vix do we appear to have sufficient evidence for more solid conclusions. In the first place, it is worth recalling that the great crater was not the only sensationally rich object in the grave of the Celtic princess. On her skull was found a superb diadem of pure gold, which consists of a plain semicircle ending in delicately decorated finials, on each of which there is a small *pegasus*. To judge by the literature known to me, this beautiful object seems to have largely defeated the experts. The opinion of R. Joffroy, the publisher of the finds, which is reflected in the labelling of the Museum of Châtillon-sur-Seine, that the piece might be of Graeco-Scythian workmanship, was based on the supposed similarity of the shape of the horses of the *pegasi* to that of Przewalski's Horse.[6] In a more thoughtful contribution, Becatti noted that we have no true analogy, and

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[4] B. B. Shefton, *Die 'rhodischen' Bronzekannen* (*Marburger Studien zu Vor- und Frühgeschichte 2*, Mainz 1979).

[5] i.163.

[6] R. Joffroy, *Le trésor de Vix* (*Mon. et Mém. Fond. Piot* xlviii.1, Paris 1954; also published separately) 48. J. V. S. Megaw, 'The Vix Burial', *Antiquity* xl (1966) 38-44, gives a useful, critical, survey of the problems and of the scholarly literature down to that date, and rightly dismisses further, wild, elaborations of the Przewalski's Horse idea.

thought of an artist working in or near the Greek world specially to suit the taste of a Celtic princess.[7] Higgins contented himself with classifying the work as Graeco-Celtic, and stating that there is no true Greek jewellery in the Vix Treasure.[8] Hodson's and Rowlett's suggestion that the diadem might have come from Spain, because it was a gold-producing area,[9] seems to be a step backwards.

The categories Graeco-Scythian, Graeco-Thracian etc., are useful to the art historian,[10] but for our purposes the essential point is that these objects were made by Greeks for non-Greek customers. The detailed decoration of the finials of the Vix diadem looks perfectly Greek in quality and character. There may be no precise analogy for the diadem, but we can find illuminating comparisons, if we change the scale. The whole is like a Greek bracelet or armband greatly increased in size.[11] To follow the same line of thought, is it too fanciful to see in the terminals drop-earrings also massively increased in size?[12] Joffroy noted that, when the diadem was worn, the terminals would be at the level of the ears.[13] So we may imagine a Greek jeweller adapting his normal methods to the larger scale required by a product designed to be, not only suitable for a different taste in adornment, but also vastly more expensive. As for the place where this craftsman worked, is it too bold to take the *pegasi* as pointers to Corinth? We know that they worked in gold at Corinth, if we make the natural assumption that the bowl of the Cypselids was a Corinthian work.

The massive bronze crater is much the largest vessel that we have from antiquity (though smaller than some known from literary sources),

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[7] G. Becatti, *Oreficierie Antiche* (Rome 1955) 60f.

[8] *Op. cit.* 118, 213.

[9] F. R. Hodson and R. M. Rowlett, "From 600 BC to the Roman Conquest," *France before the Romans*, ed. S. Piggott and others (London 1974) 157-191; see 180-182.

[10] See, e.g., Higgins, 118.

[11] This shape is very common and of long ancestry in the East; see K. R. Maxwell-Hyslop, *Western Asiatic Jewellery* (London 1971) 196f., plates 149-152. A Greek example of sixth-century date was found at Gordium; see G. Kenneth Sams, 'Imports at Gordium', *Expedition* xxi.4 (1979) 6-17, at 12, where it is called Achaemenid, surely wrongly. There is another, from Cyprus, in the British Museum; see Higgins, plate 30; F. H. Marshall, *Catalogue of the Jewellery, Greek, Etruscan and Roman, in the Departments of Antiquties, British Museum* (London 1911) no. 1985.

[12] Cf. the earrings worn by the Berlin *kore*; G. M. A. Richter, *Korai. Archaic Greek Maidens* (London 1968) no. 42, pp. 39f.; though the shape is different.

[13] *Op. cit.* 47.

having a maximum height of ‖ 1.64 metres, a widest diameter of 1.27 metres, a total weight, with cover, of 208 plus kilograms, and a capacity of as much as 1,250 litres.[14] Experts have suggested numerous places for its origin: Vulci, Spina, Sparta, Rhegium or other Chalcidian colonies in the West,[15] and Corinth. This last was the choice of Gjødesen, who, in the most perceptive discussion known to me, found a number of very close parallels in Corinthian art, and argued cogently that the crater's date of production should be considerably earlier than the date of burial in *c.* 525-500, namely about 575.[16] However, the most fruitful approach to the questions how and why this grandiose object came to Vix is by looking at the non-precious goods imported from the Greek world by the Late Hallstatt oppidum at Mont Lassois.

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[14] Joffroy suggested (*op. cit.* 22, cf. 57) that the Vix crater held 300 amphorae and was thus of the same size as the Spartan gift to Croesus (Hdt. i.70). But he had to assume that these amphorae were "les petites amphores" which "ne contiennent guère plus de 3 à 4 litres" (22). The normal amphora was a well-established measurement of capacity. For instance, the Attic amphora contained about 39 litres, as we know from both literary and physical evidence, which is satisfyingly concordant; see F. Hultsch, *Griechische und Römische Metrologie*[2] (Berlin 1882) 101-109; M. Lang and M. Crosby, *Agora X. Weights, Measures and Tokens* (Princeton N.J. 1964) 59; S. Young, 'An Athenian Clepsydra', *Hesperia* viii (1939) 274-284, at 278f. (for the capacity of the *chous*, twelve of which made an amphora); cf. also A. W. Johnston and R. E. Jones, "The 'SOS' amphora" *BSA* lxxiii (1978) 103-141, at 133-135. It is obvious that, when Herodotus gives the capacity of a vessel in amphorae, he is using a standard measure, well known to all Greeks, which must be of the same order of magnitude as the Attic amphora. This means that the capacity of the Vix crater was about 30 amphorae, and was thus totally overshadowed by Herodotus' largest vessels, the Spartan gift to Croesus (300 amphorae), Croesus' silver crater at Delphi (i.51.2; 600 amphorae) and the Scythian bronze vessel made from arrow heads (iv.81; 600 amphorae). Some have regarded Herodotus' vessels as too large to be true; see, e.g., O. K. Armayor, "Did Herodotus ever go to the Black Sea?," *HSCP* lxxxii (1978) 45-62, at 49-57; but their arguments are not conclusive, and history has not been kind to those who have accused Herodotus of lying; cf. A. D. Momigliano, "The place of Herodotus in the history of historiography," *History xliii* 1-13 (1958) (= *Secondo contributo alla storia degli studi classici* (Rome 1960) 29-44).

There are two paintings of giant craters in Etruscan tombs at Tarquinii; see, P. Ducati, *Monument della pittura antica scoperti in Italia, Sezione Prima, La pittura etrusca, Tarquinii, Fasc.* 1, *Le pitture delle tombe delle leonesse e dei vasi dipinti* (Rome 1937) pp. 3f., Tav. I, II, and F. Weege, *Etruskische Malerei* (Halle 1921) fig. 79, plate 85. Both are dated to the second half of the sixth century; see Ducati, 18-21; Weege, 107. In both cases the craters reach about the shoulders of the human figures depicted in the paintings, so they represent vessels about the same size as that of Vix, which may, therefore, be regarded as a standard giant crater, but not so large as to achieve renown in the ancient world.

[15] F. Villard, *La céramique grecque de Marseille* (Paris 1960) 141f.

[16] M. Gjødesen, "Greek Bronzes: a review article," *AJA* lxvii (1963) 333-351. Gjødesen's findings were approved by Megaw, *op. cit.*

As Villard rightly noted, the crater is statistically but one number among a large quantity of Greek imports.[17] These include transport amphorae, Attic Black Figure ware and quite ordinary Massaliot table ware. Since the oppidum at Vix was connected to Massalia by a very clear geographical route up the Seine valley and down the valley of the Rhône, the pottery imports make it a natural hypothesis that the Greek goods came from or via Massalia.[18] Obviously it is conceivable that this hypothesis is wrong, just as it is conceivable that the crater and diadem, either or both, did not come by the same route as the wine and drinking vessels, but the principle of economy of explanation encourages acceptance of the conclusion that Massalia was involved in the trade with Vix, and that the precious objects also most probably reached Vix via Massalia. ||

The knowledge that the great Phocaean colony was trading with the Late Hallstatt oppidum is a gain in itself, but only a beginning. We should like to know what came to Massalia in return, who carried the goods, and, in particular, for what reason and in what way the very costly objects went to Vix. It has been argued that the Phocaeans were interested in tin from Cornwall.[19] Literary sources of the Hellenistic period bear witness to the tin route from North to South across France, a route which could have been controlled by the oppidum of Mont Lassois at that point. According to this interpretation, the precious objects would have been gifts designed to win the favour of the local rulers. This is attractive, so long as we think only of Vix, but we now know of many Late Hallstatt sites in the broad area of eastern France, southwestern Germany and northwestern Switzerland, which were importing Greek goods in the sixth century, and it is less easy to explain all this trade on the hypothesis of the tin trade.[20] We are thus left with many questions and few answers. The people of these Hallstatt oppida imported

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[17] *Loc. cit.*

[18] As Villard, *ibid.* This is much preferable to Gjødesen's argument for a route up the Adriatic and over the Alps, which depends on simply drawing the straightest line on the map between Vix and Corinth; see 345. More recently, P. S. Wells has accepted that the main route for the Mediterranean goods, which came to Late Hallstatt communities in W. Europe, was via Marseilles; see *Culture Contact and Culture Change: Early Iron Age Central Europe and the Mediterranean* (Cambridge 1980) *passim.*

[19] See, for a full statement of the case, Villard, 142-154.

[20] As P. S. Wells, "West-Central Europe and the Mediterranean," *Expedition* xxi.4 (1979) 18-24, at 21; *Culture Contact and Culture Change*, 67-70.

Greek wine, drinking vessels and *objets de luxe*; we do not know what they gave in return.[21] Nor do we know who carried the goods on their journey by river and overland. Nor can we tell whether the crater and diadem were gifts or merely special trade goods, presumably ordered individually.[22] This ignorance of rather simple, but fundamental, matters is, unfortunately, characteristic of our knowledge of the long-distance trade between Greeks and natives.

The involvement of Massalia in that trade was very probably paralleled by many other Greek colonies. For example, it is very likely that the Scythian princes received their precious Greek products through the Greek colonies of the Pontus.[23] But, if we seek to reconstruct the commercial relations of Greek colonies with native peoples with any degree of clarity and security, we must leave the dramatic trade in luxuries and at long distance, and turn to the more mundane interchanges in the immediate vicinity of the colonies.

What we find is that the Greek colonies provided a commercial centre for the neighbouring native peoples in a variety of ways. When Seuthes, a prince of the Odrysian Thracians, hired Xenophon's mercenaries in the winter of 400 BC, he used them against his Thracian enemies and victims.[24] In order to pay Xenophon's men, he turned his captives into money by selling them in the slave markets of the Greek cities of the coast.[25] Here the Greek colonies provide the services of a big, developed market, that could absorb large quantities of slaves and supply in return large quantities of coined money.

These services are so important to Seuthes that he has a regular Greek agent, Heraclides of Maronea, who conducts his business in the Greek cities, and represents his interests among the Greeks.[26] This is rather late for the period that we are discussing, but we know of another Greek agent of a local ruler at a somewhat earlier date, viz. the agent of the Scythian king, Ariapeithes, who was called

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[21] Wells suggests (*op. cit.* 67-70 and 97) various natural products: furs, pitch, resin, wax, honey, leather, textiles, meats and (?) slaves. But this is all quite hypothetical, resting as it does on probability and the analogy of products obtained by the Greeks in the Black Sea area.

[22] Diplomatic gifts are the favoured explanation nowadays, and are supported by the analogy of Sparta's gift to Croesus (Hdt. i.70); cf. Wells, 77-79. Jacobsthal thought of special orders for the splendid objects at Trebenište; see *Greek Pins*, 203.

[23] Cf. n. 2 above.

[24] Xen. *Anab.* VII.

[25] Xen. *Anab.* VII.iv.2; v.2.

[26] Xen. *Anab.* VII.iii.16; iv.2; v.2, 5f.; vi.41f.

Tymnes, was an informant of Herodotus, and probably lived in Olbia.[27] We may confidently suggest that our examples testify to a widespread phenomenon. ||

Another service often provided by Greek colonies to the neighbouring native peoples was coinage. Greek coins are frequently found in native sites in the vicinity of Greek colonies. The possibility obviously exists that these coins were valued for their precious metal, as bullion, rather than used for money. However, in some instances we know that coinage of base metal or of minimal value was in widespread use among the natives neighbouring Greek colonies. The so-called 'dolphins' or 'fish coins' of Olbia, which were made of cast bronze, are a good example; these were in use as early as the sixth century.[28] Another instance of similar date is provided by the tiny silver coins found in vast numbers in Colchis, and hence called 'Colchidians'.[29] These were almost certainly minted at the Greek colony of Phasis. Their silver content is extremely variable, which proves that they were used and valued as money not bullion. Such instances seem to show that it was not only the coins themselves, but also the use of coinage as money, that spread from the colonies to the neighbouring non-Greek peoples. Since they received their coinage from the Greek colonial mints, there is a clear element of interchange here; the natives received the benefit of coined money, but the Greeks received goods and services in exchange.

A special development in the same general category occurred when a local ruler struck his own coins at a Greek mint. This is a common phenomenon. Apart from the coins of Persian satraps, struck at East Greek mints, and coins of Illyrian kings, struck at Dyrrhachium in the early Hellenistic period,[30] we know of a Thracian king, who

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[27] Hdt. iv.76.6; see Macan's commentary *ad loc.* and xcvi n. 7; I. Vinogradov, "Die historische Entwicklung der poleis des nördlichen Schwarzmeergebietes im 5. Jahrhundert v. Chr.," *Chiron* x (1980) 63-100, at 76.

[28] A. N. Zograf, *Antichnye Monety* (Moscow/Leningrad 1951) 241f.; A. Waşowicz, *Olbia pontique et son territoire* (Paris 1975) 60f.; *eadem*, "Les indices de la civilisation et de l'hellénisation des côtes de la mer noire dans l'antiquité, *Dial. d'hist. anc.* vi (1980) 29-39, at 32.

[29] Waşowicz, *ibid.*

[30] *Catalogue of the Greek Coins in the British Museum (BMC), Thessaly etc.* p. 80 plate XIV; B. V. Head, *Historia Numorum*² (Oxford 1911) 316. The new dating (c. 350) proposed by H. Ceka, *Questions de numismatique illyrienne* (Tirana 1972) 23-27, is quite untenable; see N. G. L. Hammond, "The kingdoms of Illyria, *circa* 400-167 BC," *BSA* lxi (1966) 239-253, at 246f.

certainly used the mint of Maronea in the fourth century.[31] These examples encourage one to interpret some enigmatic fifth-century coins with Olbian types, but with inscriptions which include non-Greek names, as something similar, namely coins of local rulers struck at the mint of Olbia.[32] We cannot, unfortunately, decide whether services such as these were something demanded by a political or military superior, or whether the mint took on the additional business as a commercial venture. But, even if the former is the true explanation—and it seems the more probable—an exchange of some sort is certainly present here.

These service-type interchanges deserve attention, because they show that we have to deal with more complex links than simple exchange of goods. Nevertheless, it is such direct trade that most of our evidence attests. A particularly good example from literary evidence concerns Epidamnus. In his *Greek Questions* (No. 29) Plutarch asks, "Who is the Seller (πωλητής) among the Epidamnians?" He replies, "The Epidamnians, who were neighbours of the Illyrians, noticed that the citizens who had dealings with the Illyrians became bad (πονηροί), and, fearing a revolution, they chose for such commercial dealings and for the trading exchanges an annual officer from those of proven worth among them, who visited the barbarians and provided a market and place of sale for all the citizens. He was called the Seller." In order to keep the contacts between Greeks and natives to a minimum, therefore, this one magistrate undertakes all the dealings with Illyrians, and provides a special market for trade with them. We may confidently assume that this market was outside the city, like the markets Greek cities would provide for foreign armies, if they were not willing to allow them within the walls. The need for such measures clearly shows that the trade and commercial interchanges were considerable. As to the date of these arrangements, Plutarch almost certainly obtained his information on || Epidamnus from the Aristotelian *politeia*,[33] so they must belong to Classical or pre-Classical times.

The best archaeological evidence for such local trade is pottery, usually Greek painted pottery, found in native settlements and ceme-

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[31] Head, 282-284. Maronea struck coins for Amadocus which included the name of the local magistrate, so the use of the mint is absolutely certain.

[32] As Vinogradov, *op. cit.* 77f.

[33] See W. R. Halliday, *The Greek Questions of Plutarch* (Oxford 1928) pp. 137-139.

teries. If the precise origin of the pottery cannot be determined, it may often be confidently conjectured that it came via the most important colony in the region, but complete certainty is lacking. However, archaeologists are increasingly able to distinguish between Greek pottery imported from Greece and Greek pottery manufactured in Greek colonies. For example, the table ware of East Greek type made in Massalia can be so distinguished, and, as a result, archaeologists are able to state that much of this pottery found on sites in South France and North-East Spain originated quite definitely in Massalia itself.[34] Similarly, we are able to recognize the local ceramic production of Pithecusa,[35] and of other Greek colonies in the West.[36]

This information allows us to pinpoint the exact origin of some of the Greek goods found in native sites, which gives us one certainty in our reconstruction of the trade. When the native site in question is in the vicinity of the source of the Greek goods, intermediaries are unlikely, and direct exchanges between the Greek colony and the local people may be assumed. In a large number of instances, therefore, we know that Greek colonies were supplying the local people with material goods of various kinds—for the pottery that is such important evidence for us will have been only a part of the goods actually supplied. Massaliot amphorae, which are so frequently found at the sites of S. France and N.E. Spain,[37] attest trade in wine, for example, and we may assume that plenty of perishable goods have left no archaeological record.

But while the general character of the trade is clear enough, questions remain. Just as in the case of the long-distance trade, we have no good evidence of what the Greeks received in return. For these goods have left no trace, and scholars are left to conjecture, rather

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[34] See Villard, *op. cit.* 58-61, 119-121; J.-P. Morel, "L'expansion phocéenne en occident: dix années de recherches (1966-1975)," *BCH* xcix (1975) 853-896, at 882f. Morel also stresses local production of Massaliot types.

[35] G. Buchner, "Pithecusa: scavi e scoperte 1966-71," *Le genti non greche della Magna Grecia. Atti del undicesimo convegno di studi sulla Magna Grecia* (Naples 1972) 361-374, at 369-372.

[36] Megara Hyblaea is a particularly good, and particularly well-published instance of the identification of the local ceramic production of an Archaic Greek colony; see G. Vallet and F. Villard, *Mégara Hyblaea II. Céramique archaïque* (Paris 1964) 11, 139-141, 143-188, 189-199. On the local production of Sicilian Naxos, see P. Pelagatti, "Naxos II—Ricerche topografiche e scavi 1965-1970," *Boll. d'arte* lvii (1972) 211-219, at 218f.

[37] See Villard, *op. cit.* 120; F. Benoit, *Recherches sur l'hellénisation du midi de la Gaule* (Aix-en-Provence 1965) 182-186, with distribution of finds to date (186).

vaguely, raw materials, agricultural products or other perishable goods. Slaves, in particular, are a favourite candidate,[38] for we know from literary sources that many of the areas of Greek colonization provided slaves for the Greek world. In any particular case, however, we simply cannot tell.

Other questions concern the organization of the trade and who carried the goods. The circumstances which led to the appointment of the Poletes at Epidamnus show that there large numbers of local Illyrians frequented the Greek city, or its environs, presumably buying small quantities of goods at retail for themselves. It is natural to conjecture also that people from places at a greater distance than was convenient for frequent visits would buy in larger quantities, and then themselves sell at retail in their own home communities. We rarely have evidence that bears on such questions. A small boat, the remains of a skeleton and fifteen complete bronze vessels were found in 1960 at Peschanoe, a village situated about 200 kilometres ESE of Kiev, in the valley of one of the tributaries which flow into the Dnieper from the left.[39] This is a long way inland from the coast of the Pontus, || roughly 350 km. as the crow flies. It is assumed that the boat's owner drowned in the river, together with his boat and cargo of bronzes. So this is a rare, perhaps unique, inland wreck. The vases are all fine Greek vessels dated to the early fifth century, except for one situla taken to be Etruscan and dated to the second half of the fourth century. This striking variation in the date of the goods, not to mention the high value of the cargo, perhaps differentiates this find from the more mundane local trade, which we may assume to have been common. Nor is it possible to decide the nationality of the boat's owner. An anthropologist pronounced the bones to have come from a young man of Mediterranean type, but that does not settle the nationality. Possibly the distance from the coast speaks in favour of a native rather than a Greek.

So we have little evidence and a few arguments in favour of the conclusion that Greek goods were carried by native people. It could

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[38] See M. I. Finley, "The Black Sea and Danubian Regions and Slave Trade in Antiquity," *Klio* xl (1962) 51-59; S. Humphreys, "Il commercio in quanto motivo della colonizzazione greca dell'Italia e della Sicilia," *Riv. Stor. Ital.* lxxvii (1965) 421-433, at 429.

[39] O. D. Ganina, *Antichni Bronzi z Peschanovo* (Kiev 1970). I work from the Russian summary, 95-97. Cf. also P. Alexandrescu, "Les importations grecques dans les bassins du Dniepr et du Boug," *Rev. Arch.* 1975, 63-72, at 67.

be conjectured, no doubt, that Greeks were also carrying goods to the native communities, but there are, fortunately, some archaeological finds from Italy, which allow us to go beyond conjecture.

High up the river valleys of Lucania, in the hinterland of Metapontum, lies the hill town of Garaguso. In two short campaigns of excavation in 1969 and 1970, J.-P. Morel tested the settlement and dug some tombs and two votive deposits.[40] His period II, from *c.* 560 to *c.* 450, is the one that interests us. The settlement and tombs concurred in revealing a community, which was clearly native, but imported some Greek vases, though these were in a definite minority compared with native wares, both fine and coarse. However, the votive deposits told a different story. Here the pottery was almost exclusively Greek, there were masses of Greek terracotta figurines and face-masks, metal objects, including the votive keys, which we know from the Greek sanctuary of Foce del Sele, and coins of Taras, Metapontum, Sybaris, Croton, Caulonia and Posidonia. Bones of small animals bore witness to sacrifices. This looks like clear evidence of Greek worship. To it we must add the small but superb goddess in a naiskos, made of marble, which was discovered at the same site at an earlier date, and which has usually been recognized as a cult image.[41]

Morel thought that these testimonies to Greek religious practices showed that the native inhabitants had taken over Greek forms of worship.[42] He rejected the interpretation that the worshippers might have been Greeks, on the grounds that no Greek writing was found, the offerings were of mediocre quality, and the figurines seemed to be eclectic, which suggested, he thought, that the worshippers were insouciant about the identity of the deity.

But are these arguments sufficient to counterbalance the clearly Greek character of the rites? As for the deity or deities concerned, we have no idea how many altars there may have been. And both the lack of writing and the mediocrity of the offerings could be explained by the fact that this was a fairly remote little place. Finally, the cult image was far from mediocre. If we prefer, as I do, to see this worship as signalling the presence of Greeks, the variety of the

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[40] "Garaguso (Lucania). Traditions indigènes et influences grecques," *CRAI* 1974, 370-395.

[41] Morel, 373 with n. 1.

[42] 393-395.

coins may suggest that the place served as a stopping-place for Greek merchants travelling by land from colonies on the coasts of the Gulf of Tarentum and the Tyrrhenian Sea. If we may compare small things with great, Naucratis and Gravisca show the importance of Greek cults to Greek merchants.

The interpretation suggested here for the finds at Garaguso seems to be strengthened by those from another Italian site, which has been investigated more recently.[43] This is Oria, the ancient Hyria, which is at the midpoint of the land route from Taras to Brindisi, and was, for Herodotus (vii. 170.2), the most ancient Messapian/ Iapygian city, founded by wandering Cretans, who had accompanied Minos to Sicily. ||

Here a cult site developed round a cave, which was situated just outside the wall circuit of the ancient settlement. The excavation discovered a large dump of votive material, which showed that the first period of active use of the sanctuary lasted from the middle of the sixth century to the first decades of the fifth. Apart from bones of small animals, there was plenty of fine Greek pottery, terracotta figurines of Tarentine type representing a goddess on a throne wearing the *polos*, bronze and silver fibulae, silver plaques with gorgon masks or in the form of acorns, and incuse coins of Metapontum, Croton, Caulonia and Sybaris. This material has been recognized as similar to the votive deposits at Garaguso.[44] One difference, however, is that this time a fragment of a vase of the end of the sixth century bore part of the name of the dedicator, a Greek name written in Greek.[45] So here we quite definitely have Greeks dedicating at a sanctuary in native territory.

It is finds such as these which help us to trace the movements of those who took part in the commercial interchanges between Greek colonies and the neighbouring natives. Only by patient attention to such humble details are we likely to unravel the complicated problems involved in reconstructing Greek trade. ||

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[43] F. D'Andria, "Salento arcaico: la nuova documentazione archeologica," *Salento Arcaico. Atti del colloquio internazionale—Lecce 5-8 Aprile 1979 (Università di Lecce. Quaderni dell'Istituto di Archeologia e Storia Antica 1*, Galatina 1979) 15-28; on Oria, see 27f. See also C. Pagliara, "Materiali iscritti arcaici del Salento," in the same volume, 57-91.

[44] See Morel's remarks in the discussion, 116.

[45] See Pagliara, 86f. The Achaean alphabet is revealed by the san, and by the iota, which was first written three-stroked and then corrected into a single vertical stroke.

4. CORINTHIAN COLONIES AND THUCYDIDES' TERMINOLOGY

Among the evidence used to determine the relationship between Corinth and her colonies are certain expressions used to describe the colonies by ancient authors. Kahrstedt[1] used them to support his thesis that the colonies were outlying parts of the Corinthian state, their citizens no different from Corinthian citizens, and he is followed by Will;[2] while Hampl[3] was led by them to see in the Corinthian colonies examples of what he called 'Poleis ohne Territorium', a view accepted by Gschnitzer.[4] But although this evidence is regularly adduced it does not seem to me that it has ever been thoroughly tested, in order to try to find out what the expressions really imply.

The expressions in question describe the colonies as 'of the Corinthians' (Κορινθίων πόλιν and the like). They are listed by Kahrstedt (p. 358)[5] and include one passage from Demosthenes (IX.34) and three from Thucydides. That from Demosthenes is in a rhetorical list of Philip's outrages, so it is perhaps best not to press his description of Ambrakia and Leukas as Κορινθίων but rather to confine the investigation to the Thucydidean passages. Thucydides' descriptions might be expected to have the more precise significance. He calls Anaktorion Κορινθίων πόλιν (IV.49), Sollion Κορινθίων πόλισμα (II.30.1), and Chalkis Κορινθίων πόλιν (I.108.5). To draw valid conclusions from these expressions it is necessary to examine Thucydides' usage in some detail.

The examples in Thucydides of the word πόλις or a place name combined with an ethnic in the genitive plural divide into two main categories. First there is the expression ἡ τῶν δεῖνα πόλις, which is a periphrastic way of describing the actual city of the people in the genitive. Thucydides' practice seems to be to use this periphrasis

[1] *Griechisches Staatsrecht*, Göttingen 1922, 357ff.

[2] See *La Nouvelle Clio* VI 1954, 413ff., especially 414f., and *Korinthiaka*, Paris 1955, 522f.

[3] *Klio* XXXII 1939, 39ff.

[4] *Abhängige Orte im griech. Altertum* (*Zetemata* 17), Munich 1958, chapter 23.

[5] Those in Thucydides were previously noted by E. Harrison, *CQ* VI 1912, 101f.; 177.

Historia 10 (1962), 246-252

where he wants to distinguish between the city proper and the territory of the state in general. Thus in the passage VIII.95.3 the words τῆς τῶν Ἐρετρίων πόλεως designate the city proper, in order to make exact the distance that he is measuring. Similarly in the passages VIII.34 and VIII.38.2 the city of Chios is distinguished from the island by the wording, and the phrase τὴν πόλιν τῶν Κυθηρίων (IV.54.1) presumably also expresses the difference between the whole island and the city of Kythera.[6]

His varying descriptions of Teos may be susceptible of a similar interpretation. We know that a distinction between the city of Teos and its land could be felt before the time in question from phrases in the inscription Tod 23 B: ἐν Τέωι ἢ γῆι τῆι Τηΐηι (9f.); τὴ[ν] πόλ[ιν καὶ γῆν] τὴν Τηΐων (12f.). Thucydides writes that Strombichides sailed ἐς Τέων (VIII.16.1), and destroyed the wall τῆς Τηΐων πόλεως τὸ πρὸς ἤπειρον (16.3). However, Tissaphernes later destroyed τὸ ἐν τῇ Τέῳ τεῖχος, if any remained (20.2), and at 19.3 some ships escaped ἐς τὴν Τηΐων πόλιν, although a sentence earlier they fled ἐπὶ τῆς Τέω. But the lack of distinction is only apparent. The wall has been described accurately in the || earlier passage (16.3), so its position could be more generally ἐν τῇ Τέῳ at 20.2; while the ships fled in the direction of Teos, but actually made good their escape to the city of Teos.

The second category embraces those expressions where the place is clearly not the actual city of the people in the genitive. Panakton is perhaps described in this way (V.3.5): εἷλον δὲ καὶ Πάνακτον Ἀθηναίων ἐν μεθορίοις τεῖχος Βοιωτοὶ ὑπὸ τὸν αὐτὸν χρόνον προδοσίᾳ. But it is very awkward to combine Ἀθηναίων with Πάνακτον, rather then with τεῖχος or μεθορίοις, so it is unlikely that it should be included among the examples. However, as the sentence might conceivably be construed to give the meaning "Panakton of the Athenians, a fort on the border," it is worth looking at Panakton's legal position. The only evidence for this is from Thucydides, who writes (V. 42.1) that the Boeotians destroyed it on the excuse that both sides (themselves and the Athenians) had sworn in the past μηδετέρους οἰκεῖν τὸ χωρίον ἀλλὰ κοινῇ νέμειν. The last word is translated in Liddell and Scott (s.v. νέμω) as to occupy or inhabit or possess, but the scholiast takes it in the sense to graze or pasture. The scholiast's interpretation is to be preferred, firstly because it makes a good contrast between μηδετέρους οἰκεῖν and κοινῇ νέμειν; on the other inter-

[6] Cf. Skylax 46: Κύθηρα νῆσος καὶ πόλις καὶ λιμήν.

pretation this contrast is not made clear, since νέμω in this sense has a similar meaning to οἰκέω; secondly, the meaning to graze suits the countryside of the frontier between Attica and Boeotia. But the strongest reason for preferring the scholiast's interpretation is that in all other passages where Thucydides uses νέμω in the sense to possess etc. he uses the middle voice. Panakton is therefore to be seen as disputed territory on which the Athenians had built a fort.

For the present purpose, therefore, it is at best a doubtful example which reveals little. Thucydides' expression Μινῴαν τὴν Μεγαρέων νῆσον (IV.67.1) is more helpful. The topography of Minoa has been studied recently by Beattie,[7] who makes a fairly good case for believing that at some time in antiquity Minoa may have been both a headland and an island. However he cannot hope to convince many with his argument that this is implied in Thucydides' statement (III.51.3), which must continue to be understood as meaning that Minoa was an island attached to the mainland by a bridge. Beattie argues that Thucydides' bridge was not attaching the island to the mainland, but at some point further inland, for Nikias had taken some of the mainland opposite and "there could have been no question of enemy troops attacking the island until they had first overrun his position on the mainland" (p. 26). But Thucydides' words are plain: ἀπετείχιζε καὶ τὸ ἐκ τῆς ἠπείρου, ᾗ κατὰ γέφυραν διὰ τενάγους ἐπιβοήθεια ἦν τῇ νήσῳ οὐ πολὺ διεχούσῃ τῆς ἠπείρου. Nikias simply occupied the mainland end of the bridge, an obvious precaution. The bridge is still the approach to the island, and Thucydides' word ἔσπλουν (*ibid.*) does not seem to exclude this obvious interpretation. The channel could have been navigable (as Beattie says, p. 26) without offering a way right through.

In spite of some topographical uncertainty,[8] therefore, we can say that Thucydides is describing an island, which was sufficiently near to Megara to be much better than Salamis as a point from which to launch an attack on Megara (III.51.2), and which was attached to Megarian territory by a bridge (III.51.3; IV.118.4). The Megarians had built a tower on it and used it as a fort. For the present purpose there seems little difficulty here. Thucydides' words describe an island which counted as Megarian territory, but because it was an

[7] *RhM.* 103, 1960, 25-8.

[8] For the topographical problems in general see, in addition to Beattie's article, Gomme, Commentary to III.51.1.

island it was not part of the Megarid. We do not know if the island was inhabited, but it was clearly not in any sense a city. ||

Thucydides describes Myonnesos with the phrase Μυοννήσῳ τῇ Τηΐων (III.32.1). Its position and nature are established by late sources. Livy gives the best description of its topography (XXXVIII.77.7),[9] and Strabo's information (XIV.643) that the Teians in the second century persuaded the Romans not to allow Myonnesos to be fortified against them shows that it was a place separate from Teos at that time. The only evidence for its status in earlier times is the statement that it was called a polis by Hekataios.[10] The evidence allows no certainty, but Thucydides' expression here may describe political control by Teos of a non-Teian city and population.

The topography of Torone has been convincingly elucidated by Meritt,[11] and it is in the light of his conclusions that Thucydides' descriptions are to be understood. In the first place the phrase τὴν μὲν οὖν ἄλλην πόλιν τῶν Τορωναίων (IV.110.2) is to be explained by the fact that before Brasidas' arrival there were effectively two cities at Torone, the city proper, and another situated at about three stades distance around the sanctuary of the Dioskouroi (IV.110.1), which Thucydides calls a suburb (προάστειον) in another passage (V.2.4). Thus the words τὴν . . . ἄλλην πόλιν τῶν Τορωναίων describe in effect part of the city of Torone, and this instance is nearer to those in the first category, where the phrase described the actual city of the people in the genitive. However, another passage about Torone belongs to the category under discussion.

Some three kilometres to the south of Torone was an enclosed harbour called Κωφὸς λιμήν.[12] Thucydides writes (V.2.2) that Kleon sailed ἐς τὸν Κωφὸν λιμένα τῶν Τορωναίων ἀπέχοντα οὐ πολὺ τῆς πόλεως.[13] The order of words makes it awkward to take πόλεως with Τορωναίων,[13a] though if the sentence is so construed we should have an example like that of Eretria discussed above, where the periphrastic construction is used to indicate the city proper in a measurement of distance (VIII.95.3). It is more natural to take Κωφὸν λιμένα with τῶν Τορωναίων, and to understand the phrase as describing a harbour belonging to Torone. It is uncertain whether there was any set-

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[9] For a modern description see *AM* XXIX 1904, 228.
[10] See Steph. Byz. s.v. Μυόννησος.
[11] *AJA* 27, 1923, 451ff.
[12] See Meritt's map (452) and discussion (454ff.).
[13] See Gomme, Commentary *ad loc.*, for the necessary emendation Κωφὸν.
[13a] As Gomme notes *ibid.*

tlement at Κωφὸς λιμήν,[14] and there was another harbour, much less sheltered, at Torone itself, where, for example, the Athenian triremes lay in 424.[15] Thucydides' words are, therefore, to be understood as a description of a small outlying piece of territory belonging to Torone. The position of the island Minoa off Megara, which Thucydides described in the same terms, is closely comparable.

Pheia is described by Thucydides (VII.31.1) as Φειᾷ τῇ Ἠλείων, a phrase which may be compared with his other description of it as τῆς Ἠλείας (II.25.3). This is a particularly interesting instance because there is considerable evidence for the status of the communities subordinate to Elis.[16] Ancient evidence other than Thucydides[17] confirms that in very general terms "Pheia always counted as part of Elis."[18] More precisely it may be assumed to be part of the *perioikis* of Elis.[19] A general account of the subjection of this territory by Elis is given by Strabo (VIII.355), and when the Spartans demanded that the Elians || should release the *perioikoi*,[20] they refused to do so on the grounds that they had won the cities in war.[21] Although there are considerable uncertainties, the evidence for the status of the communities of the Elian *perioikis* suggests that they were not incorporated into the Elian state.[22] Their payment of tribute[23] is perhaps

[14] Meritt found no sherds; see map p. 452.

[15] See Thuc. IV.113.2 and Meritt's map.

[16] This evidence is given and discussed by Gschnitzer, *Abhängige Orte im griech. Altertum*, chapter 2.

[17] See Xen. *Hell.* III.ii.30; Polyb. IV.9.9 (in spite of the strange νῆσον on which see Walbank, Commentary *ad loc.*); Diod. XII. 43.4.

[18] *RE* s.v. Phea 1912.

[19] As *RE* s.v. Elis 2422, though Thucydides does not say so expressly in the passage II. 25. 3.

[20] Xen. *Hell.* III. ii. 23.

[21] ἐπιληΐδας ἔχοιεν τὰς πόλεις. Although the word ἐπιληΐς only occurs here, it is clearly to be understood as war-booty or the like; cf. Gschnitzer, *Abhängige Orte*, 12.

[22] Gschnitzer, in a long discussion (chapter 2), decides that they formed separate communities on Elian territory, but much of his evidence is of doubtful value. The inscriptions from Olympia which he adduces (10f.) to show their separate existence are not exactly dated, and, as he admits, might come from a period of independence. Nor does the appearance of the ethnic Μακιστίος (Xen. *Anab.* VII. iv. 16) show that the Makistians had a separate legal existence. Legal conclusions from the existence of ethnics are inadmissible. Thucydides, for instance, can call Samian exiles in Anaia Ἀναιιτῶν (III.19.2; cf. IV.75.1 and Gomme, *Commentary* I.375 n. 3). Furthermore the description of a Lepreate victor at the Olympic Games in 424 as Ἀλεῖος ἐκ Λεπρέου is an awkward fact which Gschnitzer can only remove by some special pleading (16f.). But his difficulties probably arise from the attempt to achieve too precise legal definitions.

[23] The statement in Strabo (VIII.355) justifies the view that Thucydides (V.31.2) gives an example of a payment regularly imposed on these communities.

the clearest indication that they were not so incorporated, and this is not offset, as has been thought, by evidence that they were liable to military service.[24] Thus Pheia was a place subject to Elis, won by military expansion, and this is the situation lying behind Thucydides' description.

Mylai in Sicily is described by Thucydides as Μυλὰς τὰς Μεσσηνίων[25] (III.90.2). Mylai was founded by Zankle early in its own history, at the end of the eighth century, at a distance of about twenty-five miles from the mother city.[26] Its relationship to Messene/Zankle has been universally assumed[27] to be that of a fort; it is seen simply as an extension of the mother city by a completely subordinate settlement. The positive evidence for this view is provided by the descriptions in ancient authors, to which may be added the negative evidence that it did not issue coins and apparently had no oikist. The most informative literary evidence is the passage Thuc. III.90.2f., which deserves detailed attention.

The Athenians attacked Mylai, defeated with great loss two tribal regiments of the Messenians, which were garrisoning the town, then turned on the fort and made the people agree to surrender the acropolis and join in an attack on Messene. This account || seems to show that although the Messenians were in control of Mylai, its inhabitants were not themselves Messenians. If they had been they would have been indistinguishable from the two regiments of Messenians who were garrisoning the town; and it seems unlikely that Messenians would have been forced to fight Messenians.

[24] The duty of military service was inferred from Thucydides' words (II.25.3): οἱ δὲ Ἀθηναῖοι . . . σχόντες τῆς Ἠλείας ἐς Φειὰν . . . καὶ προσβοηθήσαντας τῶν ἐκ τῆς κοίλης Ἤλιδος τριακοσίους λογάδας καὶ τῶν αὐτόθεν ἐκ τῆς περιοικίδος Ἠλείων μάχῃ ἐκράτησαν. See, e.g., *RE* s.v. Elis 2422. But the Elians from Pisatis are probably, as Gomme suggests (Commentary *ad loc.*), Elians who were living there, rather than Pisatai. Gschnitzer (p. 7 n. 5), on second thoughts, accepts Gomme's view.

[25] τὰς τῶν in some manuscripts.

[26] See Bérard, *La colonisation grecque de l'Italie méridionale et de la Sicile dans l'antiquité*, 2nd edition, Paris 1957, 97. The date 717/6 has been assigned to the colony by attributing Eusebius' notice of the foundation of Chersonnesos in Sicily to Mylai. The title fits the geographical position: see Bérard 98. But the attribution is hypothetical, and is rejected by Vallet, *Rhégion et Zancle*, Paris 1958, 83. However, he shows (84) that the archaeological finds suggest that its foundation was very close in date to that of Zankle, at latest at the end of the eighth century.

[27] See, e.g., Dunbabin, *The Western Greeks*, Oxford 1948, 211, Bérard 97f., Ziegler *RE* s.v. Mylai 3, 1042, Vallet 117.

A recent discovery at Olympia seems to support these inferences from Thucydides, and to clarify our ideas about the relations between Messene and Mylai at the time in question. The object is a Corinthian helmet inscribed Μεσσένιοι Μυλαίον.[28] Although a full publication is still awaited, from what we are told of the letter forms[29] a date not later than the first half of the fifth century would seem reasonable. Although the inscription is brief to the point of obscurity, there is an analogy for it, also a dedication at Olympia. This is the bronze greave inscribed Δανκλαῖοι Ῥεγίνον, published by E. Kunze and H. Schleif in *II. Bericht über die Ausgrabungen in Olympia* (1937/8) 69f. The editors dated the object to the first years of the fifth century, and interpreted the inscription, the very brief form of which was then without parallel, as meaning that the people of Zankle were dedicating booty taken in a war against Rhegion. As a dedication at the same sanctuary by people from the same area this parallel may be used to interpret the new discovery, and we may conclude that the Corinthian helmet inscribed Μεσσένιοι Μυλαίον will have been dedicated as war booty taken by the Messenians from the people of Mylai.

The form of the name Μεσσένιοι is significant. From the numismatic evidence it appears that this form of the ethnic was used from the time when Anaxilas changed the name from Zankle to Messene until about 460 BC, when the spelling Μεσσάνιοι became regular.[30] At some time, therefore, within this period the Messenians were involved in a struggle with Mylai. It would not be over audacious to suggest that this struggle was an appendage to the seizure of the city of Zankle by Anaxilas and its resettlement with new settlers.[31] Presumably their success at Zankle was followed by the capture of Mylai.

The helmet also gives us far the earliest evidence for the ethnic Μυλαῖος, not attested otherwise, according to Ziegler, before Diodoros (XII.54.5).[32]

It seems, therefore, likely that Thucydides' description reflects the status of Mylai after the events attested by the dedicated helmet.

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[28] Reportedly briefly in *Archaeological Reports for 1959-60*, published by The Hellenic Society and the British School at Athens, 1960, p. 11, and in *BCH* 83, 1959, 655f.

[29] *BCH loc. cit.*

[30] See Robinson, *JHS* LXVI 1946, pp. 18 and 20. Robinson's treatment of the coins is largely followed by Vallet 338ff.

[31] For the evidence on these events, and full discussion, see Vallet 335ff.

[32] *RE* s.v. Mylai 3, 1042. His reference to Diod., XIX.65.3 seems to be a mistake.

The Messenians have control of the city, but its inhabitants are Mylaians, not Messenians.

It may be seen from the analogies that have been discussed that a literally parallel expression to the descriptions of Chalkis, Sollion and Anaktorion as Κορινθίων πόλιν etc. is not found in Thucydides. Phrases like τὴν πόλιν τῶν Χίων (VIII.34), though verbally nearly parallel, are clearly not so in meaning, as has been seen, and this difference in meaning is expressed by the significant verbal distinction that πόλις in these descriptions always has the definite article. So the effective parallels are those of the second category. Among these we have seen places of varying status vis-à-vis the controlling state. Minoa and Κωφὸς λιμὴν were small outlying pieces of territory, perhaps scarcely inhabited; Pheia definitely, Mylai probably and perhaps Myonnesos were separate though dependent communities. On these analogies strict conclusions about legal status cannot be drawn from Thucydides' descriptions of Corinthian colonies; the parallels merely prove that the descriptions reflect effective Corinthian control. It would therefore seem wrong to conclude ‖ that the citizens of the Corinthian colonies were citizens of Corinth simply on the grounds of Thucydides' descriptions.

However, it should be remembered that passages precisely analogous in words and meaning are not found. This not only means that conclusions based on these analogies must be tentative, but is itself significant. Thucydides' normal way of describing a colony, found very frequently, is ἡ τῶν δεῖνα ἀποικία. If he did not use it on these occasions it was because he wanted to express something more than origin. A truly precise analogy of relevant date is Herodotus' description of Stryme as Θασίων πόλις (VIII.108). The status of the mainland colonies of Thasos has been made clearer recently by a new inscription.[33] This records two laws encouraging delation, probably to be assigned to the year 411 BC,[34] and shows that Thasos could legislate for her colonies in a way which at first sight would suggest that they were parts of the Thasian state.[35] Yet other evidence, which

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[33] Published by J. Pouilloux, *Recherches sur l'histoire et les cultes de Thasos I*, Paris 1954, no. 18, pp. 139ff.

[34] Pouilloux' arguments for this date, both epigraphical and historical, seem convincing. In his long and detailed review of Pouilloux' book (REG LXXII 1959, 348ff.) Chamoux stresses the uncertainties of Pouillox' reconstructions and interpretations of epigraphical documents. But on the laws encouraging delation his criticisms carry little weight.

[35] See the opening of the second law: ὃς δ' ἂν ἐν τῇς ἀποικίῃσιν ἐπανάστασιν

shows their separate existence, excludes this conclusion. Two, Galepsos and Neapolis, paid tribute separately to Athens,[36] and Neapolis is clearly treated by the Athenians as a separate community in the document IG I² 108.[37] Further, the Athenians were able to make Thasos give up her mainland possessions after the revolt (Thuc. III.50.2); they would have been difficult to detach if they had been legally indistinguishable from the state of Thasos. The evidence, therefore, seems to point to very close political control by the metropolis over colonies which still formed separate communities. If Herodotus' words imply this state of affairs, we have perhaps an analogy which contributes to our understanding of Thucydides' expressions.

One final caveat. The descriptions that we have discussed refer only to Anaktorion, Sollion and Chalkis. Leukas, on the other hand, is called τὴν Κορινθίων ἀποικίαν (I.30.2), one of Thucydides' normal ways of describing a colony, as are Ambrakia (II.80.3), Apollonia (I.26.2) and Potidaia (I.56.2). The status of Sollion and Chalkis is unknown, but Anaktorion was in Corinthian hands by capture at the time to which Thucydides is referring. Perhaps the special control implied by his words is not universally applicable to all the tyrant foundations of Corinth.

The conclusions of this investigation are largely negative, or at least limiting. The analogies, unsatisfactory though they are, do not support the theory that the colonies were part of the Corinthian state. For in none of them, where people were clearly involved, are they likely to have been citizens of the controlling state. As for the theory that the colonies were "Poleis ohne Territorium," I have recorded general objections to this elsewhere.[38] It also seems quite likely that the implications of the expressions studied || should only be applied to the colonies so described. In sum we should conclude that far-reaching generalisations from the expressions here discussed are unjustified. But this negative conclusion has positive value. For it should lead to a truer evaluation of the whole body of evidence for the relations between Corinth and her colonies. ||

βολευομένην κατείπηι, ἢ προδιδόντα τὴν πόλιν Θασίων τινὰ ἢ τῶν ἀποίκων, καὶ φανῆι ἐόντα ἀληθέα, διηκοσίος στατῆρας κτλ.

[36] See *ATL* I Register s.v. Νεάπολις παρ' Ἀντισάραν and Γαλήψιοι.

[37] Republished by Meritt and Andrewes in *BSA* XLVI 1951, 200-209, whose version is now *SEG* XII.37.

[38] See *JHS* LXXX 1960, 221. On the same subject cf. the critical remarks of Habicht in *Gnomon* XXXI 1959, 705f.

5. THE HISTORICAL INTERPRETATION OF AL MINA

Al Mina has been regarded as a Greek colony founded by Euboeans, and it was once stated that the Greek town there "has been proved by excavation to have been no less important a project than that of the earliest western colonies."[1] To this supposed Greek colony has been attributed the key role in the transmission of the oriental influences that affected the Greek world at the end of the Dark Ages. Not only oriental goods and raw materials, but alphabetic writing and oriental myths have all been thought to have come to Greece from Al Mina.[2] It has also been stated that Euboeans, "having possession of Al Mina in the East and Pithekoussai in the West. . . . were, from about 775-700 BC, the masters of trade between the Eastern Mediterranean and Central Italy."[3] Seen as a very early Greek colony from Euboea, Al Mina clearly takes on great importance in the history of Greek colonization in general. However, other views have been taken of its character. Coldstream has recently described it as "the Levantine emporium founded in the late ninth century . . . containing a small enclave of Greeks from the beginning,"[4] while Culican stated, "I would regard Al Mina as an offshoot of metropolitan Phoenicia, and I exactly call it Phoenician."[5] The range of interpretation is disturbingly wide; what is the evidence?

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[1] J. Boardman, Early Euboean Pottery and History, *BSA* 52 (1957) 1-29, at 24f.

[2] Cf. J. N. Coldstream, Greeks and Phoenicians in the Aegean, *Phönizier im Westen*, ed. H. G. Niemeyer (*Madrider Beiträge* 8, Mainz 1982) 261-272, at 262. On alphabetic writing, see L. H. Jeffery, *Local Scripts of Archaic Greece* (Oxford 1961) 11f.; R. M. Cook and A. G. Woodhead, The diffusion of the Greek Alphabet, *AJA* 63 (1959) 175-8; and, recently, L. H. Jeffery still writes (of the place of takeover), "Al Mina remains a promising candidate," *CAH* III.1², 822. On oriental myths, see P. Walcot, *Hesiod and the Near East* (Cardiff 1966) 121f.

[3] G. Buchner, Pithekoussai, oldest Greek colony in the West, *Expedition* 8 (1966) 4-12, at 12; quoted with approval by D. Ridgway, The first Western Greeks: Campanian coasts and southern Etruria, *Greeks, Celts and Romans*, ed. Christopher and Sonia Hawkes (London 1973) 5-38, at 20.

[4] *Phönizier im Westen*, 262; compare his *Geometric Greece* (London 1977) 93, and contrast his *Greek Geometric Pottery* (London 1968) 357, where, even if with some reserve, he accepted the idea of the Euboean foundation of Al Mina.

[5] *Phönizier im Westen*, discussion, 79. A brief expression of my own opinion was given at *CAH* III. 3², 93f., where space did not allow the issues to be examined at length. Cf. also Niemeyer's remarks on the "Al Mina effect," Die Phönizier und

Al Mina ('the port' in Arabic) is the name of a site excavated by Woolley in the two seasons of 1936 and 1937.[6] It is more precisely a low mound, called Sheik Yussuf, near the mouth of the Orontes. Woolley found it in his search for the port that he inferred must have been controlled by the Bronze Age kingdom of Alalakh, higher up the river in the Amuq plain, the kingdom that he investigated at the mound called Atchana.

True to his preconceptions, Woolley assumed that there had been a Bronze II Age harbour town at Al Mina, all traces of which he thought to have been washed away by the river, but what he actually found was an entirely Iron Age settlement. It is an ironical situation that Woolley, an orientalist interested in early civilizations, was not primarily concerned with the period after 1200 BC. Iron Age Al Mina was thoroughly subsidiary to his chief aims and receives short shrift in his book, *A Forgotten Kingdom*, where his attitudes were ingenuously revealed: "Of the work at the harbour, al Mina. . . . here it is enough to say that, unfortunately, all vestiges of its early settlement had been swept out to sea by the Orontes changing its course . . ., all that we found on the site dated to long after 1200 BC, a period with which . . . I had felt we were not concerned; it is perhaps humiliating to have to confess that the results were some of the most interesting obtained by the expedition."[7]

Woolley divided his stratigraphy at Al Mina into ten levels, from I, the highest, most recent, to X, the lowest, most ancient. Of these I was the Crusader Town, while II-X belonged to the ancient period.[8] However, Woolley himself repeatedly confessed that he often had

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die Mittelmeerwelt im Zeitalter Homers, *Jahrb.d.römisch-germanischen Zentralmuseums* 31 (1984) 3-94, at 27.

[6] Woolley published the following accounts of his excavations at Al Mina: Excavations near Antioch in 1936, *Antiquaries' Journal* 17 (1937) 1-15; Excavations at Al Mina, Sueidia I and II, *JHS* 58 (1938) 1-30, 133-70; *A Forgotten Kingdom*, first edition (London: Pelican Books, 1953), revised edition (London: Max Parrish, 1959); the page references here are to the revised edition. The Greek pottery was published by J. D. Beazley, Excavations at Al Mina, Sueidia III. The Red-figured vases, *JHS* 59 (1939) 1-44, and by M. Robertson, Excavations at Al Mina, Sueidia IV, The Early Greek Vases, *JHS* 60 (1940) 2-21. The coins from the first (the main) season's digging, by E. S. G. Robinson, Coins from the excavations at Al Mina (1936), *Num.Chron.* 5th series, 17 (1937) 182-96; and the non-Greek pottery by J. Du Plat Taylor, The Cypriot and Syrian Pottery from Al Mina, Syria, *Iraq* 21 (1956) 62-92.

[7] P.6. For his brief account of Al Mina, see 153-67.

[8] *Forgotten Kingdom*, 154f.; *JHS* 1938, 6-7.

great difficulty in distinguishing between these levels,[9] and experts who have studied the material since have sometimes found it difficult to reconcile with his stratigraphy. Robertson, for instance, noted that the pottery of the early period shows many cross-level joins,[10] while a level that Woolley thought shortlived, VIII, was regarded by J. du Plat Taylor as long, with significant architecture.[11] There is, therefore, room for doubt and disagreement, and it is not likely that, with our present evidence, all the problems can be solved.[12]

The earliest levels, X, IX and VIII, the first of which rested on virgin soil,[13] seem to belong together chronologically. Very little remained of X and IX,[14] and the Greek pottery of all three levels seems to be similar.[15] There is some disagreement, however, about level VII. J. du Plat Taylor thought it belonged with IX and VIII architecturally,[16] while Boardman considered it quite nugatory, and stated that, if it was abolished, there would be no change in the stratigraphy.[17] Coldstream, however, felt that the way the sherds combine forces him, 'faute de mieux', to make a break at the beginning of VII.[18]

A more fundamental problem is presented by the absolute chronology of these early levels, especially the question when the settlement found by Woolley at Al Mina began. The early commentators, Woolley himself, for instance, and Robertson, chose an eighth-century date.[19] However, J. du Plat Taylor finally settled on *c.* 825,[20] and she has been widely followed.[21] Her chronology was founded upon

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[9] See, e.g., *JHS* 1938, *ibid.*, 10, 142-4, 150, 154f. It is difficult to read these statements without disquiet.

[10] *JHS* 1940, 2.

[11] Woolley, *Forgotten Kingdom*, 161; J. Du Plat Taylor, *Iraq* 1959, 85.

[12] There are useful discussions of the levels and chronology in Boardman, *The Greeks Overseas*,[3] (London: Thames and Hudson, 1980) 39f.; Tarsus, Al Mina and Greek Chronology, *JHS* 85 (1965) 5-15; Coldstream, *Greek Geometric Pottery*, 310-16; and J. Du Plat Taylor, *Iraq* 1959, 62-92.

[13] *JHS* 1938, 16, 155.

[14] *Ibid.*

[15] Robertson, *JHS* 1940, 2 with n. 1.

[16] *Iraq* 1959, 91f.

[17] Greek potters at Al Mina? *Anat. Stud.* 9 (1959) 163-9, at 167 n. 15.

[18] *Greek Geometric Pottery*, 313.

[19] *Forgotten Kingdom*, 155; Robertson, *JHS* 1940, 2-4.

[20] *Iraq* 1959, 91f.

[21] E.g. Boardman, *Greeks Overseas*[3], 43f.; Coldstream, *Phönizier im Westen*, 262 with n. 7.

a high dating of the non-Greek pottery, which is not universally accepted.[22] Whatever may be the truth of this matter, it does seem hard to achieve a precise chronology of the non-Greek wares. The Greek painted pottery, especially the skyphoi with pendent concentric semicircles (PSC), might be expected to offer a closer date. Here too, however, we have a form of decoration with a very long life, from before 850 down to 700, or possibly even later.[23] On the face of it, therefore, these skyphoi would seem to accommodate J. du Plat Taylor's high date easily enough. However, the very careful analysis of the typology of these vessels, published recently by R. Kearsley,[24] seems to provide us with criteria by which we can establish the approximate date of individual PSCs with some confidence. She examined "all the psc skyphos fragments from Al Mina that are in England, and only one fragment can now be assigned a date earlier than 750."[25] That fragment belongs to a type dated by Kearsley to the first half of the eighth century. These questions of pottery chronology in the ninth and eighth centuries are complicated and difficult, and a non-expert is well-advised not to pronounce upon them. Nevertheless, it seems to me that a ninth-century date for the beginning of the settlement at Al Mina may be too high.

It is easier to feel confidence in the lower terminus for the early period at Al Mina. At the end of the eighth century we have a dividing point (either between levels VIII and VII, or, possibly, between VII and VI), which has been attributed to an Assyrian

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[22] See E. Gjerstad, The stratification at Al Mina (Syria) and its chronological evidence, *Acta Arch.* 45 (1974) 107-13, who attacks, in particular, the Cypriot chronology of J. Birmingham; see The Chronology of some Early and Middle Iron Age Cypriot Sites, *AJA* 67 (1963) 15-42. This is similar to that of J. du Plat Taylor. However, some later discoveries seem to harmonize well with Birmingham's chronology; see V. Karageorghis and L. Kahil, Témoignages eubéens à Chypre et chypriotes à Érétrie, *Antike Kunst* 10 (1967) 133-5, and, in general, P. M. Bikai, *The Pottery of Tyre* (Warminster: Aris and Phillips, 1978). W. Culican, though he thinks that Gjerstad's chronology may be "perhaps too severe in its low dating," nevertheless advocates dates considerably lower than those of J. du Plat Taylor; The repertoire of Phoenician Pottery, *Phönizier im Westen*, 45-82, at 68.

[23] See the full and well-documented treatment by Kearsley, in J.-P. Descoeudres and R. Kearsley, Greek Pottery at Veii: another look, *BSA* 78 (1983) 9-53, at 41-52.

[24] *Ibid.*

[25] *Ibid.*; see especially n. 146. Kearsley examined 25 fragments from Al Mina; see 49-52 with n. 148. Where the level was recorded, they came from IX-VIII or VIII. The pottery from X remained in Antioch, and has not, to my knowledge, been published; cf. Robertson, *JHS* 1940, 2 n. 1.

destruction,[26] when a break in occupation may have occurred.[27] Levels VII, VI, V (or VI and V) cover the seventh century, after which there is a long break, perhaps of eighty years, before the rebuilding of the town on new lines in the later sixth century (level IV).[28] Levels IV and III cover the period from the later sixth century to the end of the first quarter of the fourth. III was a rebuilding of IV in *c.* 430 on the same lines, and the dating of both levels was well established by Attic pottery.[29] Since the buildings of level III were destroyed by fire, this was the most fully preserved level of the mound. The latest ancient level, II, covered the period from *c.* 375 up to *c.* 300, when occupation of the site ceased.[30] This date for the end of the site's occupation in ancient times was well confirmed by the coins.[31] Level II was very near the surface and much destroyed by the plough.[32]

On the basis of the well-preserved buildings in level III, some of which were warehouses containing trade goods,[33] Woolley thought that the whole town, throughout its history, consisted of such port offices, which were rebuilt and remodelled from time to time as necessary. He also believed that the people who worked in the town mostly lived in the settlement on the hill Sabouni, some five kilometers further inland, where he dug some test trenches in 1936 (but the finds were not published).[34] This rather extravagant and fragile hypothesis is partly refuted by the presence of intramural burials in levels II-IV, which Woolley himself conceded must attest habitation.[35]

The site was rich in finds, especially pottery, and much of this pottery is Greek. Woolley decided that the merchants of his town must have been Greeks,[36] and that their settlement was the Posideum

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[26] Cf. J. Du Plat Taylor, *Iraq* 1959, 85f.
[27] See Boardman, *Greeks Overseas*,[3] 39.
[28] *Ibid.* and J. Du Plat Taylor, *Iraq* 1959, 85f., 87, 91f.
[29] Woolley, *Forgotten Kingdom*, 164-6; *JHS* 1938, 21, 24.
[30] Woolley, *Forgotten Kingdom*, 167; *JHS* 1938, 6, 26.
[31] Robinson, *Num. Chron.* 1937, 182.
[32] Woolley, *JHS* 1938, 133.
[33] *JHS* 1938, 14f., 140.
[34] *JHS* 1938, 8f.; Sidney Smith, The Greek Trade at Al Mina, *Antiquaries Journal* 22 (1942) 87-112, at 88-90. Woolley re-asserted his convictions in The date of al Mina, *JHS* 68 (1948) 148, a brief note referring to further finds at Sabouni in 1947-8.
[35] *JHS* 1938, 148, 155-7; *Forgotten Kingdom*, 158.
[36] *JHS* 1938, 15: "These merchants must have been Greeks."

briefly mentioned by Herodotus (3.91.1) as founded by Amphilochus.[37] These contentions raise two questions, which are different: 1) Was the settlement at Al Mina a Greek city? 2) Were there Greek inhabitants at Al Mina? ||

Al Mina has no Greek graves and no evidence for Greek cults. The architecture of the rambling warehouses has been described as "more Phoenician than Greek in character,"[38] and the fact that the roofs were not tiled throughout the history of the settlement[39] surely shows that the buildings were not Greek. In bulk the non-Greek pottery equalled the Greek throughout the eighth century, and the minor objects at Al Mina are mostly non-Greek.[40] In the Classical period, in levels which yielded quantities of fine Greek pottery, the presence of intramural burials attests a practice totally unGreek.[41] There is also a strong general argument from comparisons. The finds in the early levels at Al Mina are closely comparable with many other contemporary sites in the Levant and Cyprus, and place Al Mina firmly in the common Cypro-Levantine cultural area of the early Iron Age.[42]

In view of all these negative indications, it was very important for Woolley's interpretation to identify his site with Posideum, a presumed Greek city. There was a chronological problem in such an identification, since the legendary foundation of Posideum by Amphilochus would point to the late Bronze Age, whereas the site Woolley discovered begins on virgin soil in the early Iron Age. However, as we have seen, Woolley always argued that there had been a Bronze settlement at Al Mina, all traces of which had been swept away by the Orontes, an article of faith which he regarded as triumphantly confirmed by the Bronze Age pottery found in his trials on the hill Sabouni.[43] Whether or not these arguments of Woolley have any validity—and they seem impossible for us to judge with the evidence available—[44] any chronological problem in identifying Al Mina with

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[37] *Forgotten Kingdom*, 159f.; *JHS* 1938, 28-30.

[38] Coldstream, *Geometric Greece*, 93; P. J. Riis, Griechen in Phönizien, *Phönizier im Westen*, 237-60, at 246.

[39] Woolley, *Forgotten Kingdom*, 155; *JHS* 1938, 11.

[40] Boardman, *Greeks Overseas*,[3] 44; J. Du Plat Taylor, *Iraq* 1959, 62-92.

[41] Woolley, *JHS* 1938, 13 with the plans of levels III and IV, 148, 157; *Forgotten Kingdom*, 158.

[42] J. Du Plat Taylor, *op. cit.*, especially 91; cf. also Culican (n. 5 above) and J. Birmingham, *AJA* 1963, 24f.

[43] See n. 34 above.

[44] For Sidney Smith's strong criticism of the theory, see *ibid.*

Posideum is relatively unimportant, since the topographical reasons against such an identification are overwhelming.

Ancient evidence suggests that Posideum lay considerably further South than Al Mina, and the case for its identification with Ras-el-Basit, which was made long ago, seems very strong.[45] Apart from the possible similarity of the modern name, Strabo (16.751) mentions Posideum when progressing South from the mouth of the Orontes and Mount Casius. But the decisive evidence seems to be provided by part of the bulletin of Ptolemy III, which is preserved on papyrus (*FGH* 160). This refers to the year 246 BC, and describes his operations in the Third Syrian War.[46] Column II lines 16-22 tell of his sailing to Posideum on one day and spending the night there, before sailing on to, and arriving at Seleucia, on the next day. Since he anchored at Posideum about the eighth hour, there were still about four hours of daylight remaining on the first of these days. From Basit to Seleucia is about 37 kilometers, from Al Mina to Seleucia about seven. It is surely right to follow those who have argued that Ptolemy's movements strongly suggest that Posideum should be at Ras-el-Basit rather than Al Mina.[47] Basit has a good harbour,[48] and we now have plenty of evidence of an ancient city there from the excavations of recent years.[49] These finds dispose of the counter-argument that no ancient remains have been || found at Basit.[50]

We may therefore regard the identification of Al Mina as Posideum as a misguided theory. It is worth noting, finally, that legends of heroic foundations did not make the Iron Age cities of Cilicia and North Syria Greek.[51]

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[45] See the good arguments assembled by R. Dussaud, *Topographie historique de la Syrie antique et médiévale* (Paris 1927) 418f.

[46] See E. Will, *Histoire politique du monde hellénistique* I (Nancy 1966) 223f.

[47] P. J. Riis, *Sukas* I (Copenhagen 1970) 137f.; Sidney Smith, *Antiquaries' Journal* 1942, 97f.

[48] As Riis and Dussaud show, *locc. citt.*

[49] See P. Courbin, Ras el Bassit, Al Mina et Tell Sukas, *Rev. Arch.* 1974, 174-8; *Annales Arch. Arab. Syr.* 22 (1972) 45-59; 23 (1973) 25-38; 26 (1976) 63-9; 27-28 (1977-8) 29-40.

[50] T. J. Dunbabin, *The Greeks and their Eastern Neighbours* (*JHS* Suppl. Paper 8, London, 1957) 26. The identification of Basit and Posideum is accepted by T. Braun, *CAH* III.3^{2}, 10.

[51] Cf. J. M. Cook, *The Greeks in Ionia and the East* (London 1962) 61-5; *CAH* II. 2^{3}, 795f.

In answer to the first of our questions, therefore, we may conclude that neither the archaeological finds nor any literary evidence suggest that Al Mina was a Greek city.

In attempting to answer our second question—whether there were Greek settlers at Al Mina—we may look first at the evidence for Greek writing. One of Woolley's arguments in favour of identifying his merchants as Greeks was that the inscriptions he had found were Greek.[52] These inscriptions were five, or, possibly, four. Three of them were amphora stamps, found in levels II and III.[53] In level II a fragment of a white marble slab was found, which bears parts of four lines in "late lettering."[54] The cursive forms certainly point to a date in the Roman imperial period (the stone came from very near the surface). It seems possible that a similar, smaller fragment of marble, bearing three letters of similar type, which was found in level III,[55] was part of the same inscription. It is obvious that none of these inscriptions has any bearing on the question whether there were Greeks resident at Al Mina.

Woolley seems also to have believed that the brief graffiti on vases of Classical date were Greek.[56] A recent study of these has shown, however, that the vast majority of the numerous, very short and enigmatic inscriptions, which are incised on Greek vases of the late fifth and early fourth centuries, are Phoenician. There is a minority in Aramaic, and, possibly, one in Greek.[57]

A lengthier ownership inscription on a vase of the same period from Al Mina is certainly Greek.[58] Until recently, this was the earliest Greek inscription known from the site,[59] and it was thought that there was no epigraphic evidence, in any language, for the ethnic origin of the inhabitants in the Archaic period.[60] However, a short

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[52] *JHS* 1938, 15.

[53] See *JHS* 1938, 166, MNN 41, found in level III; 164, MNN 120, 140, both found in level II; all three illustrated in fig. 20, p. 145.

[54] *JHS* 1938, 163, MN 59.

[55] *JHS* 1938, 166, MN 48; illustrated in fig. 20, p. 145.

[56] *JHS* 1938, 15.

[57] See F. Bron and A. Lemaire, Inscriptions d'Al-Mina, *Atti del I Congresso Internazionale di Studi Fenici e Punici* (Rome 1983) 677-86.

[58] See J. D. Beazley, Ten inscribed vases, *Arch. Eph.* 1953/4, 200-6, at 205f., no. 10 fig. 5.

[59] Cf. Riis, *Phönizier im Westen*, 240f.

[60] Cf. my statement, *CAH* III.3^2, 93.

time ago there was discovered the first piece of evidence for writing from that period at Al Mina.[61]

This is a small fragment of pottery, identified as from the wall of a skyphos, which is recorded as coming from levels VI-VII, which dates it, *grosso modo*, to the seventh century. On it there are five letters, or parts of letters, written from left to right, which seem to be reasonably certainly Greek. The three letters which can be read are αβε. This is not a common sequence. Apart from the aorist forms of λαμβάνω and the perfects of some compounds of verbs beginning with β (as καταβάλλω, διαβαίνω), it only appears in a restricted number of Greek words, none of which seems very likely to appear in a vase graffito.[62] Not surprisingly, therefore, it has been suggested that the graffito represents the remains of a proper name, γ̣αβεο̣.[63]

What can this rather unhelpful scrap tell us? Not that Al Mina was a place where Greek was written in the alphabet at a very early date. It is too late for ll that.[64] Nor that the writer came from some specific part of the Greek world.[65] The proper conclusion would seem to be that this sherd makes it probable that someone wrote Greek at Al Mina in the seventh century BC (though it is not impossible that the vessel was inscribed before it reached the coast of Syria). So most of the little evidence for writing at Al Mina is of no help in answering the question whether there were Greeks at Al Mina in the Archaic period, but this new discovery possibly makes a small contribution.

Most scholars have assumed that there were Greek inhabitants on the basis of the Greek pottery. Thus Rüs thought that the abundance of Greek pottery at Al Mina made the assumption of a Greek colony admissible,[66] and Coldstream sees "a gradual infiltration of

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[61] See J. Boardman, An inscribed sherd from Al Mina, *Oxford Journal of Archaeology* 1 (1982) 365-7.

[62] A rapid test by Ibycus computer of some Greek authors in *TLG* revealed the following: ἀβέβαιος, ἀβέλτερος, ἀστραβές, βλαβερός, εὐλάβεια (εὐλαβέομαι), Κάβειροι.

[63] See Boardman, *ibid.* The dot under the last letter is mine, but seems to be required; the first letter seems very uncertain.

[64] Boardman, *ibid.*, argues that the undistinctive sherd is Attic Late Geometric, dismissing the implications of the level where it was found.

[65] Boardman tentatively proposes Aegina, but the shapes of three letters in so informal an inscription hardly permit such deductions.

[66] Riis, *Sukas* I, 159; cf. *Phönizier im Westen*, 253, where he is not sure if Al Mina

merchants from the Aegean," a few of whom may have resided from the beginning, arguing from the quantity of Greek sherds, which he calls 'a handful' in the earliest level, increasing to the majority in the seventh century.[67]

The assumption that Greek painted pottery attests Greek settlers depends on the dogma that Greek pottery was not acceptable to other peoples in the East, i.e. that it was not there an object of trade.[68] On the basis of this principle, Greek settlers have been postulated in the Levant, wherever a considerable quantity of Greek pottery has been found. So Rüs found early Greek settlements of one sort or another all along the Levantine coast.[69] It is difficult to see why this principle was adopted. Even in Egypt there is a substantial quantity of Greek pottery outside the Greek settlements.[70] In any case, the appearance of Greek pottery in abundance at sites where Greek settlers are most improbable, such as Tyre, for instance,[71] not to mention Phoenician sites in the West, such as Carthage, Motya and the colonies on the South coast of Spain,[72] shows that we can no longer follow the simple rule that Greek pottery implies Greek settlers. Greek painted pottery was an object of trade, and was used by plenty of people other than Greeks.[73]

So we have to look for arguments for Greek settlers other than

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was an *apoikia*, but still thinks it could be a planned Greek settlement. The interpretation of A. Sogomonov follows similar lines of argument; Greek settlements in the Near East and their ethnocultural contacts in the Archaic period, *VDI* 1985, 1, 8-25.

[67] *Geometric Greece*, 93; followed by Braun, *CAH* III. 3², 9-11. Popham objected to the term 'handful' as a description of the Greek sherds in the earliest level; see M. Popham, H. Hatcher, A. M. Pollard, Al Mina and Euboea, *BSA* 75 (1980) 151-61, at 151 n. 1.

[68] See Sidney Smith, *Antiquaries' Journal* 1942, 96; R. M. Cook, Die Bedeutung der bemalten Keramik für den griechischen Handel, *Jahrb. deutsch. arch. Inst.* 74 (1959) 114-23, at 122.

[69] *Phönizier im Westen*, 237-60 *passim; Sukas* I, 158ff. Riis was justifiably criticised for his use, or rather abuse, of this so-called principle by Coldstream, in his review of *Sukas* I and II; *AJA* 79 (1975) 155f.

[70] See M. S. Venit, Laconian Black Figure in Egypt, *AJA* 89 (1985) 391-8, especially Appendix 2, 397-8.

[71] P. M. Bikai, *The Pottery of Tyre.*

[72] See Boardman, *Greeks Overseas*,³ 211f.; B. B. Shefton, Greeks and Greek Imports in the South of the Iberian Peninsula. The archaeological evidence, *Phönizier im Westen*, 337-70, at 338f.; *die 'rhodischen' Bronzekannen* (*Marburger Studien zu Vor- und Frühgeschichte*, 2; Mainz, 1979) 49, n. 95. These sites are also comprehensively treated by Niemeyer, *Jahrb. d. römisch-germanischen Museums* 31 (1984) 3-94.

[73] As Coldstream, *Geometric Greece*, 94f.

the presence of Greek painted pottery. If Boardman's suggestion, that a certain class of Greek skyphoi, dated to the eighth century, was made at Al Mina, were correct, we should have a good argument for the presence of Greek inhabitants.[74] Although the suggestion was presented tentatively, it has received widespread acceptance, and the class is sometimes referred to as Al Mina Ware.[75] However, the theory depends on some rather finely-drawn technical considerations, which may seem insufficient to bear the weight of the conclusion, especially as a simpler assumption is available, namely that these skyphoi were all made in Cyprus, as some of them certainly were.[76] This is the conclusion which Boardman himself now accepts as most probable.[77]

We are left with arguments from analogy and probability. Two sites on the Levantine coast, which may be compared with Al Mina, have been excavated in modern times, Tell Sukas and Ras-el-Basit. At the latter there is plenty of II Greek pottery, which begins in the eighth century, but there is nothing in the published material known to me, which definitely attests the presence of Greek settlers.[78] At Tell Sukas the best evidence for Greek settlers is provided by the spindle whorl incised Πεσαϙορες ἐ͂μι,[79] and by some graves which seem to be of Greek type.[80] Although the woman's name troubled the Roberts,[81] the inscription is indisputably Greek. It is unfortunate that its date is not very clearly established. The whorl was found in a level which cannot be dated earlier than the third century BC, but is of a type current from the eighth to sixth centuries. It was, therefore, dated by letter forms, tentatively, *c.* 600.[82] This inevitably very approximate dating appears now to have become, by a process all too familiar, definite and established.[83] The graves taken to be of

[74] *Anat. Stud.* 1959, 163-9.

[75] E.g. G. Ploug, *Sukas* II (Copenhagen 1973) 16; Coldstream, *Greek Geometric Pottery*, 384.

[76] Boardman, *op. cit.* 166f.; Coldstream, *Phönizier im Westen*, discussion, 256.

[77] *Greeks Overseas*,[3] 41f.

[78] For the publications of the excavations at Ras-el-Basit, see above, n. 49.

[79] *Sukas* I, 158, fig. 35d; *Sukas* II n°. 424, p. 90, plate XIX, figs. f. and g; Riis, *Phönizier im Westen*, 240f.

[80] *Op. cit.* 249f.

[81] *Bull.* 1967 no. 634.

[82] Riis, 'L'activité de la mission danoise sur la côte phénicienne en 1963, *Annales Arch. Arab. Syr.* 15.2 (1965) 57-82, at 59-61.

[83] Ploug, *Sukas* II n°. 424, p. 90; Riis, *Phönizier im Westen*, 240f.

Greeks have a covering of tiles in the Greek manner, contain Greek drinking vessels among the grave goods and are mostly oriented roughly E-W.[84] The orientation and grave goods are hardly decisive, but the covering of tiles is definitely a Greek practice.[85] The earliest of these graves are of the late seventh century.

This evidence shows that there were at least some Greek inhabitants of Tell Sukas from approximately the late seventh century.[86] So here we have Greek settlers in what is regarded as a Phoenician town.[87] Since we are in a similar area to that of Al Mina, and since the historical circumstances may also be assumed to be similar, it seems justifiable to use the arguments of analogy and probability and conclude that it is probable that there were some Greek inhabitants of Al Mina in the Archaic period. The newly discovered graffito discussed above would suit such an assumption.

This modest and rather vague conclusion makes some of the questions that have been asked about Al Mina of dubious historical importance. There is a debate, for example about the origin of the PSCs that were found at Al Mina. Some take them to be of Euboean manufacture,[88] others from elsewhere.[89] Attempts have been made to settle the matter by scientific analysis of the clay, but the divergent results achieved do not increase one's confidence that a proved conclusion is attainable by this method.[90] If the origin of these cups was likely to determine who founded the settlement at Al Mina, it would indeed be of great historical importance, but if, as I believe, these were minor trade goods, not necessarily even carried by Greeks, their

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[84] *Phönizier im Westen*, 249f.

[85] See D. Kurtz and J. Boardman, *Greek Burial Customs* (London 1971) 97f., 192.

[86] The notion of earlier Greek settlers rests on very slender foundations; see Coldstream, *AJA* 79 (1975) 155f. Riis still maintains that his sanctuary is Greek; see *Phönizier im Westen*, 246-9; but there are very solid objections to such a view; see, in addition to Coldstream, Boardman, *Gnomon* 42 (1972) 214-7.

[87] See Ploug, *Sukas* II, 98f.; Riis, *Sukas* I, 129, 158. Compare also Riis' significant statement that he thinks the cooking ware is Phoenician; *Phönizier im Westen*, discussion, 258.

[88] Boardman, *Greeks Overseas*,[3] 41 and *BSA* 1957, 1-29; Popham, *BSA* 1980, 151-61, and in M. Popham, H. Hatcher, A. M. Pollard, Euboean exports to Al Mina, Cyprus and Crete, *BSA* 78 (1983) 281-90.

[89] Coldstream, *Geometric Greece*, 93f. (cf. also his statement that this class of skyphoi was copied from an early date in Cyprus; *Phönizier im Westen*, discussion, 256); Kearsley, *BSA* 1983, 52.

[90] Popham, Hatcher and Pollard, *BSA* 1980, 151-61; 1983, 281-90. I should like here to express my thanks to Dr. Harold Dibble, of the University Museum, University of Pennsylvania, for valuable advice on this subject.

place of origin is no more than a small problem in the history of Greek ceramics.

If the port at Al Mina was not a Greek settlement, who were its founders? In the present state of our knowledge we probably cannot answer this question with certainty. Culican's opinion that it was a Phoenician settlement was itself based on an argument from pottery: the Red Slip pottery from there "has the metropolitan Phoenician Red Slip."[91] In this he finds Al Mina different || from sites nearby. If we are hesitant to base our opinion on such arguments from pottery, it is, nevertheless, worth noting that the same combination of Greek, Cypriot and Phoenician pottery, which was found at Al Mina, is also found at Phoenician sites in the Levant and Cyprus, as also in the West, in North Africa, Spain and at Mogador on the West coast of Morocco.[92] The factor common to these widely separated places was the Phoenicians. As stated above, the architecture at Al Mina has also been seen as characteristic of Phoenician settlements.[93] So the archaeological evidence suggests that the Phoenicians have a good claim to be regarded as the founders of Al Mina.

But we are not restricted solely to archaeological evidence in considering this question. By an original and perceptive re-examination of Assyrian records, G. Kestemont has demonstrated that, in the ninth and eighth centuries, the Assyrian kings received tribute from Tyre and Sidon, when they were in the North Syrian region, and that, in the contemporary lists of places supplying labour or tribute to Assyria, Tyre, Sidon and Byblus occur in the geographical context of Cilicia and North Syria.[94] These interesting facts show, as he argues convincingly, that North Syria and Cilicia were areas of such importance to the Phoenician cities, that they were ready to protect their interests there by paying tribute to Assyria, even when their own home cities were not directly threatened. As Kestemont notes,[95] this evidence is entirely in harmony with that from Karatepe, which demonstrates so strikingly the importance of the Phoenician presence in Cilicia during—not to become involved in detailed chronological

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[91] *Phönizier im Westen*, discussion, 79.

[92] For the Levant, see J. Du Plat Taylor, *Iraq* 1959, 87-91; Bikai, *Pottery of Tyre*; for the West, the works cited in n. 72 above.

[93] See above p. 54.

[94] Le commerce phénicien et l'expansion assyrienne du IXe-VIIIe s., *Oriens Antiquus* 11 (1972) 137-44.

[95] P. 143.

controversy—the same general period.[96] The marked Phoenician influences at Zinçirli, which lies on the Syrian side of the Amanus range, shows that their presence was not confined to Cilicia.[97] We should also recall that it was in the ninth century that the Phoenicians, by the foundation of Citium, established themselves powerfully in Cyprus.[98]

It seems to me probable, therefore, that the true historical significance of the site at Al Mina lies in the context of Phoenician expansion the North-East corner of the Mediterranean. It is interesting to observe that Barnett could write, in reference to the general area of Cilicia and the Syro-Hittite Aramaean states in the Dark Ages, "Phoenician cultural influence was very powerful in this area, and probably radiated from some local colony not yet discovered,"[99] and Kestemont could assert, "Les Phéniciens doivent être solidement installés aux abords de l'embouchure de l'Oronte,"[100] without either author even mentioning the name of Al Mina. To such an extent has the identification of Al Mina as a Greek colony distorted historical interpretation.

Given the limited and inarticulate sources at our disposal, the historical interpretation of Al Mina is inevitably speculative, and is likely to remain so. The site does not seem to have been well dug; the material was inadequately || recorded; and it has never been comprehensively and satisfactorily published. In these circumstances, there is likely to be continuing disagreement, and I do not deceive myself that all the propositions of this paper will meet with universal acceptance. It is important, therefore, to emphasize, in conclusion, how far the historical interpretation of Al Mina has changed since the heady ideas of the early days. From Woolley's "These merchants must have been Greeks,"[101] Dunbabin's "there is nothing to differentiate the place from one of the many Greek colonies in Italy and Sicily, or on the Black Sea coast,"[102] and the quotation from early Boardman

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[96] For discussion of the Karatepe inscriptions and their historical significance, see Machteld M. Mellink, Karatepe. More light on the dark ages, *Bibliotheca Orientalis* 7 (1950) 141-50; R. D. Barnett, *CAH* II.2^3, 363-6; Irene J. Winter, On the problems of Karatepe, *Anat. Stud.* 29 (1979) 115-151.

[97] Winter, *op. cit. passim*; Barnett, *CAH* II.2^3, 442.

[98] See Coldstream, *Geometric Greece*, 66; V. Karageorghis, *CAH* III.1^2, 523.

[99] *CAH* II.2^3, 442.

[100] *Oriens Antiquus* 1972, 143.

[101] *JHS*, 1938, 15.

[102] *The Greeks and their Eastern Neighbours*, 25.

at the beginning of this paper, we move to a more circumspect later Boardman: "The town at Al Mina was not . . . wholly Greek. It is likely that there was a settlement there . . . when the Greeks began to visit the port and established a small community there."[103] Now, even those who believe in a Greek Al Mina, such as Riis, are cautious about defining it as a Greek colony.[104] Woolley's identification of his site with Posideum would probably today be defended by no one. It should be recognized, therefore, that there is little left of the interpretation which made Al Mina a uniquely early Greek colony in the East, and a uniquely important place for contacts between Greece and the Orient. The site has little or nothing to do with the great movement of Greek colonization, and is only one of many that attest close contacts between Greeks and Phoenicians.[105] It is the contention of this paper that the notion of "the Euboean emporium" should go the same way as the rest of the misguided historical interpretation of Al Mina. ||

Résumé

The settlement excavated by Sir Leonard Woolley at Al Mina was identified with the ancient city of Posideum and interpreted as an exceptionally early Greek colony in the East, which played the leading role in the relationship between Greece and the Orient at the end of the 'Dark Ages'. The bases for such an interpretation were always either very fragile or non-existent. It is now generally recognized that Posideum should be placed further South, probably at Ras-el-Basit. The material finds and architecture of Al Mina clearly attest, not a Greek city, but a Levantine settlement, probably founded by Phoenicians.

The concept of a Greek Al Mina arose from misinterpretation of the presence of abundant Greek painted pottery, which is evidence for trade not inhabitants. Analogy, probability and a very small number of graffiti suggest that there were some Greek residents, but the historian should realise that Al Mina was just one of many places in the eastern Mediterranean, where Greeks made contact with the Orient. ||

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[103] *Greeks Overseas*³, 43.

[104] *Phönizier im Westen*, 253.

[105] See Coldstream, *Phönizier im Westen*, 261-72 *passim*.

6. THE AUTHENTICITY OF THE ΟΠΚΙΟΝ ΤΩΝ ΟΙΚΙΣΤΗΡΩΝ OF CYRENE

Θεός. Τύχα ἀγαθά.
Δᾶμις Βαθυκλεῦς ἦιπε · περὶ ὧν λέγοντι τοὶ Θηραῖο[ι]
Κλευδάμας Εὐθυκλεῦς, ὅπως ἁ πόλις ὀρθῶται ὁ δ[ᾶ]
μος εὐτυχῆι ὁ Κυραναίων, ἀποδόμεν τοῖς Θηραίοις τ
ὰμ πολιτήιαν κατὰ τὰ πάτρια, τὰ οἱ πρόγονοι ἐποιήσαν
το, οἵ τε Κυράναγ κα[τώ]ικιξαν Θήραθε καὶ οἱ ἐν Θήραι [μέ]
νοντες, καθὼς Ἀπόλλων ἔδωκε Βάττωι καὶ τοῖς Θηρ[αί]
οις τοῖς κατοικίξασι Κυράναν εὐτυχὲν ἐμμένοντας το[ῖς]
ὁρκίοις, τὰ οἱ πρόγονοι ἐποιήσαντο αὐτοὶ ποτ' αὐτός, ὅκα
τὰν ἀποικίαν ἀπέστελλον κατὰ τὰν ἐπίταξιν τῶ Ἀπό[λ]
λωνος τῶ Ἀρχαγέτα · ἀγαθᾶι τύχαι · δεδόχθαι τῶι δάμω[ι]·
καταμεῖναι Θηραίοις ἴσαμ πολιτήιαν καὶ ἐγ Κυράναι κ[α]
τὰ τὰ αὐτά· ποιεῖσθαι δὲ πάντας Θηραίους τὸς ἐπιδημέ[ον]
τας ἐγ Κυράναι τὸν αὐτὸν ὅρκον, ὅμπερ τοὶ ἄλλοι ποτ
ẹ δịώρκωσαν · κ̣α̣ὶ̣ καταστᾶμεν ἐς φυλὰν καὶ πάτρα ἐς θε
ἐννῆα ἑταιρήας. Καταγράφεν δὲ τόδε τὸ ψάφισμα ἐν στάλ[αν]
λυγδίναν, θέμεν τὰν στάλαν ἐς τὸ ἱαρὸν πατρῶιον τῶ
Ἀπόλλωνος τῶ Πυθίω, καταγράφεν καὶ τὸ ὅρκιον ἐς τὰν στάλ[αν],
τὸ οἱ οἰκιστῆρες ἐποιήσαντο καταπλεύσαντες Λιβύανδε [σὺ]
μ Βάττωι Θήραθε Κυράνανδε. Τό κα ἀνάλωμα τὸ δέηι ἐς τ[ὸν λ]
ᾶον ἢ ἐς τὰγ καταγραφάν, οἱ ἐπιστάντες ἐπὶ τὸς ἀπολόγος [κο]
μισάσθων ἀπὸ τῶν Ἀπόλλωνος προσόδων.
Ὅρκιον τῶν οἰκιστήρων.
[Ἔ]δοξε τᾶι ἐκκλησίαι· ἐπεὶ Ἀπόλλων αὐτομάτιξεν Β[άτ]
τωι καὶ Θηραίοις ἀποι[κίξαι] Κυράναν, ὁριστὸν δοκεῖ Θη[ραί]
[ο]ις ἀποπέμπεν ἐς τὰν [Λιβ]ύαν Βάττομ μὲν ἀρχαγέτα[ν]
[τ]ε καὶ βασιλῆα· ἑταίρους δὲ τοὺς Θηραίους πλέν· ἐπὶ τᾶι ἴσα[ι]
[κ]αὶ τᾶι ὁμοίαι πλὲν κατὰ τὸν οἶκον, υἱὸν δὲ ἕνα, καταλ[έ]
γεσθαί τ[ε ἀπὸ τῶγ χώρων ἀπάντων] τοὺς ἡβῶντας, καὶ τῶν [ἄλ]
[λ]ων Θηραίων ἐλευθέρος, [ὅ κα λῆι], πλέν. Αἰ μὲν δέ κα κατέχ[ων]
τι τὰν οἰκισίαν οἱ ἄποικοι, τῶν οἰκείων τὸγ καταπλέον[τα]
ὕστερον εἰς Λιβύαν καὶ πολιτήιας καὶ τιμᾶμ πεδέχ[εν]
καὶ γᾶς τᾶς ἀδεσπότω ἀπολαγχάνεν. Αἰ δέ κα μὴ κατ[έχ]
ωντι τὰν οἰκισίαν μηδὲ οἱ Θηραῖοί μιν δύνανται ἐπικουρέ
ν, ἀλλὰ ἀνάγκαι ἀχθῶντι ἔτη ἐπὶ πέντε, ἐκ τᾶς γᾶς ἀπίμ[εν]
ἀδιέως Θήρανδε ἐπὶ τὰ αὐτῶγ χρήματα καὶ ἦμεμ πολιάτ
ας. Ὁ δέ κα μὴ λῆι πλὲν ἀποστελλοίσας τᾶς πόλιος, θανά[σι]
μος τένται καὶ τὰ χρήματα ἔστω αὐτοῦ δαμόσια. Ὁ δὲ ἀπ
οδεκόμενος ἢ ἀδήιζων ἢ πατὴρ υἱὸν ἢ ἀδελφεὸς ἀδελ

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φεὸν παισεῖται ἅπερ ὁ μὴ λέων πλέν. Ἐπὶ τούτοις ὅρκια ἐπ
οιήσαντο οἵ τε αὐτεῖ μένον[τ]ες καὶ οἱ πλέοντες οἰκίξοντε
ς καὶ ἀρὰς ἐποιήσαντο τὸς ταῦτα παρβεῶντας καὶ μὴ ἐμ
μένοντας ἢ τῶν ἐλλιβύαι οἰκεόντων ἢ τῶν αὐτεῖ μεν
όντων. Κηρίνος πλάσσαντες κολοσὸς κατέκαιον ἐπα ||
ρεώμενοι πάντες συνενθόντες καὶ ἄνδρες καὶ γυναῖκ
ες καὶ παῖδες καὶ παιδίσκαι· τὸμ μὴ ἐμμένοντα τούτοις
τοῖς ὁρκίοις ἀλλὰ παρβεῶντα καταλείβεσθαί νιν καὶ κα
ταρρὲν ὥσπερ τὸς καλοσός, καὶ αὐτὸν καὶ γόνον καὶ χρή
ματα. Τοῖσι δὲ ἐμμένοισιν τούτοις τοῖς ὁρκίοις καὶ τοῖς
πλέοισι ἐλλιβύαν κ[αὶ] τ[οῖς μέ]νοισι ἐν Θήραι ἦμεν πολλ
ὰ καὶ ἀγαθὰ καὶ αὐ[τοῖς καὶ γό]νοις.

In the years immediately following the discovery of the 'Stele dei Fondatori' at Cyrene (*SEG* ix 3)[1] considerable work was done on it, especially on the text, though the condition of this is still not entirely satisfactory. Some attempt was also made to establish whether the ὅρκιον τῶν οἰκιστήρων, which is included in the fourth-century Cyrenaean inscription, may be taken for what it claims to be, namely a seventh-century decree of Thera arranging for the foundation of the colony at Cyrene. The two main works devoted to this were articles by Ferri[2] and Ferrabino.[3] Neither of these can be said to give a final or satisfactory picture, and in fact scholarly opinion has for the most part simply followed Wilamowitz's conclusion, given in his short note appended to the first publication,[4] that the ὅρκιον was invented for this occasion and is comparable with the later κτίσις Μαγνησίας.[5] The most that they allow the ὅρκιον is to represent a fifth-century source for Theraean history, used both by Herodotos and the composer of this document.[6] Meiggs is an exception in believing "the main substance of the document . . . to be original."[7]

[1] This follows Oliverio's text published in *Riv. di. Fil.* vi (1928) 222-32. Many of Oliverio's readings and reconstructions were seriously and sensibly challenged by Ferri, *Historia* iii (1929) 389-96.

[2] *Abh. Berl. Akad. Phil. Hist. Kl.* 1925, no. 5, 19-24.

[3] *Riv. di. Fil.* vi (1928) 250-4.

[4] *Abh. Berl. Akad. Phil. Hist. Kl.* 1925, no. 5, 38-40.

[5] See, e.g., Busolt-Swoboda, *Griech. Staatskunde* ii 1265 n. 3; Bengtson, *Griech. Geschichte.* (Munich, 1950) 90 n. 2; Chamoux, *Cyrène sous la monarchie des Battiades* (Paris, 1952) 108ff.

[6] Thus Wilamowitz, who implies this when he says that the document shows that an account of her old history existed on Thera in the fifth century, and Chamoux.

[7] Bury, *History of Greece*[3] (1951) 862.

If the document were original it would be unique as a public decree of considerable length dating from the seventh century arranging a most important historical event. The Greek epigraphist, the Greek historian and anyone with special interests in Greek colonisation must all regard a decision on the authenticity of the ὅρκιον as highly desirable. Why, therefore, has a thorough discussion of the matter not appeared? It must be either because it is so obviously a forgery that an immediate decision is possible, or because there is not sufficient evidence or material on which to base a firm conclusion. We have almost no analogous material from so early a period, nor have we, to my knowledge, analogous epigraphical texts which quote much older documents. Thus the obvious and normal arguments from analogy are denied to scholars. So it is perhaps a dangerous or thankless task to investigate the authenticity of the ὅρκιον. But since it does not seem that the document can be lightly dismissed as a forgery, an attempt at a comprehensive discussion of the problem may at least pay due recognition to its importance to any student of early Greek history, even if no absolutely clear-cut conclusion emerges. At least the ground needs clearing of false notions and the relevant considerations need to be revealed.[8]

In outline the plan of this paper is as follows. First, a discussion of the ὅρκιον and the literary tradition about Cyrene's foundation, especially Herodotos' account; with this goes closely Ferrabino's thesis, which needs refutation but has apparently not yet received it;[9] || secondly, the relevant general considerations about forgeries, documents from the early period and its history, together with arguments from analogy; thirdly, discussion of the detailed internal material of the text itself; and, finally, a concluding section which will also include further discussion of the relationship between our document and Herodotos.

The inscription *SEG* ix 3 is the record of a decree of the people of Cyrene providing that the Theraeans should have equal citizenship

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[8] I should like to take this opportunity of thanking those who read an earlier discussion of this document and helped me with suggestions and criticisms, namely Sir Frank Adcock, the late Professor A. W. Gomme, Professor A. Momigliano, and Mr R. Meiggs. I am particularly indebted to Professor A. Andrewes for painstaking and encouraging criticism. Responsibility for all errors and failings remains my own.

[9] Chamoux, 109 n. 3, does not consider Ferrabino's arguments convincing, but provides no detailed discussion of them.

in Cyrene according to the agreement which (they said) was made at the time the colony was sent out. They also arrange for the publication of this agreement with the decree; so we have the so-called ὅρκιον τῶν οἰκιστήρων, which purports to be the decree of Thera providing for the etablishment of the colony.

It can easily be shown that the character and contents of the ὅρκιον are, in general, in agreement with the literary tradition about Cyrene's foundation. The article on 'Kyrene' in *RE* was published just before the discovery of our inscription; in fact it is there regretted (col. 159) that inscriptions from Cyrene are few, and the hope is expressed that some will come to light in the new excavations. The very swift fulfilment of this hope did not, however, invalidate anything written in this article (157f.) on the foundation of the colony. The historical deductions from the partly mythical account in Herodotos, iv 145-59, that the colony was sent out under pressure of economic necessity, that Battos was its founder, that Delphi played an important part in the events—all these were confirmed by our inscription. The discussion of the relation of the decree to the literary tradition does not, therefore, involve attempts to reconcile large inconsistencies, and this agreement on fundamentals gives us at least this encouraging beginning: the principal circumstances of the enterprise may be accepted as truly represented by decree and literary tradition. But the decree could still have been invented in accordance with literary records, in which case it does not rank as an independent source and the literary tradition is only in a superficial way confirmed by it.

Ferri and Ferrabino concerned themselves with the origin of the ὅρκιον, whether it was Theraean or Cyrenaean. This is important because Herodotos' account shows differences between the traditions from Cyrene and Thera, and, since the decree purports to be Theraean, it is naturally all the more suspect if it can be shown to represent the Cyrenaean tradition. Ferri supported the ὅρκιον's claim to come from Thera, while Ferrabino attacked his work and tried to prove the origin to be Cyrenaean. The argument naturally rests on the difference in the two traditions given by Herodotos, so this difference must be examined first. Those who have interested themselves in Herodotos' sources have discussed this matter. Jacoby, for instance, *RE* Suppl. ii 436, distinguishes chapters 150-3 as Theraean and 154-6 as Cyrenaean. But I believe that greater precision can be attained, which is my excuse for treating the subject again.

The Theraean account of the oracles preceding the expedition is given in iv 150-3. This takes one from Delphi's first command to found a city in Libya (150.3), given to King Grinnos, to the time when the expedition sailed. Once, however, Herodotos has related the departure, he says (154.1) ταῦτα δὲ Θηραῖοι λέγουσι, τὰ δ' ἐπίλοιπα τοῦ λόγου συμφέρονται ἤδη Θηραῖοι Κυρηναίοισι. Κυρηναῖοι γὰρ τὰ περὶ Βάττον οὐδαμῶς ὁμολογέουσι Θηραίοισι. From this point to chapter 156.1 we are given the Cyrenaean account of the oracles and events leading up to the departure of the expedition. In this, it is true, the only real point of similarity between the two accounts is the unvarying advice of Delphi.[10]

But the point that should stand out from Herodotos' words is that it is only on this part || of the story that the two accounts diverged. It is notable that in the account of the later adventures of the colonists (157-8) Battos is scarcely mentioned. The Pythian speaks to the colonists, not to Battos alone (157.2), and this part of the story is clearly not to Herodotos τὰ περὶ Βάττον. My submission is therefore that Herodotos proceeded as follows. He found that the two accounts differed in the events preceding the departure. So he gave the Theraean version up to the departure, then interposed the Cyrenaean version, and after that related τὰ ἐπίλοιπα τοῦ λόγου, on which the two accounts agreed. There is one very strong piece of support for this interpretation of Herodotos' words. The end of the Theraean version is formed by the information that they despatched two penteconters to the island of Platea (153). In the passage 156.2, where we come to the actual departure, after the account of the Cyrenaean tradition, the information is the same: ἀπέστελλον μετὰ ταῦτα τὸν Βάττον οἱ Θηραῖοι δύο πεντηκοντέροισι. This single repetition shows exactly where the variation in the two accounts ended.[11]

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[10] The Cyrenaean account is dismissed by Chamoux, 95ff., as a collection of fictional appendages, while Legrand (*Hérodote* iv, Budé, 1945, 158ff.) suggests with reason that Herodotos' informants at Cyrene were (1) favourable to national sentiment, (2) unfavourable to the Battiads, whose rule had ended comparatively recently. This observation is relevant to the question of the name Battos, to be discussed below.

[11] Macan (*Herodotus iv-vi* vol. ii 265ff.) attempted to show the scope of the discrepancy in the traditions, but, though he is party right, I do not think he is completely so. He writes "the discrepant traditions concern only the actual foundation story of the Libyan colony, the person of the founder and the action of the first adventurers;" he then gives a perfectly adequate account of the differences and similarities in the two accounts of Battos and Delphi. But his statement (p. 266) "it is

Both Ferri and Ferrabino failed to see the true scope of the difference in the two traditions, and much of their work is spoilt by this misunderstanding of Herodotos. With this firm basis attained comparisons between the decree and Herodotos' account can be made with confidence, and we may proceed to take Ferrabino's arguments in detail (I follow Ferrabino's numbering of his arguments).

In argument 1 Ferrabino says (p. 250) that the phrase of the decree (24f.) Ἀπόλλων αὐτομάτιξεν Β[άτ]τωι καὶ Θηραίοις ἀποι[κίξαι] Κυράναν corresponds to a typical element in the Cyrenaean tradition, seen in the passages Hdt. iv 155.3 and Pind. *Pyth.* 4.60ff., whereas the tradition of Thera said that the oracle was given to Grinnos (Hdt. iv 150.2). This argument is scarcely conclusive. It is true that we are dealing here with the events about which the two traditions differed, but even the Theraean account leaves it perfectly possible, or even probable, that the Theraeans could think of Delphi's advice being given to Battos. Already in 150.3 Grinnos points out Battos to the god, and by the time he comes to sail there has been another visit to the oracle (151.1), and their choice of him as ἡγεμών and βασιλεύς (153) makes it probable that he had visited the god in person for advice like other oikists.

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not perfectly clear at what point exactly the 'Kyrenean' story is conceived by Herodotus as ceasing to conradict the 'Theraean'" is, I think, unfair to Herodotos, who, as I have tried to show, does make the matter clear. The reason for Macan's statement is his belief that the variation must go down at least to the abandonment of Platea. For the procedure whereby they left one of their number behind, the rest applying to Delphi (157.1), seems to him to be a variant account of the action described in the There an story (151.3), where Korobios is left behind by the reconnaissance party. At first sight these no doubt look like varying accounts of the same event. But if we take them as such difficulties arise. (1) Herodotos did not see them as such, for he does not include the event in the material on which the two traditions differed, the limits of which are, as has been shown, clear. (2) It would leave the Theraean account without an explanation of the move from Platea to Cyrene, whereas if we assume thaat the agreed account is resume at 156.2 πλώσαντες δὲ ἐς τὴν Λιβύην οὗτοι, both accounts tell a complete story, though the Theraean would have two occasions on which a man was left behind. A possible explanation of this might be that it was a regular procedure in stories of colonial foundations. It is better, I feel, to assume two events than to reject Herodotos' exposition of the tradition. The differences are large enough, viz.: in 151.3 Korobios is left behind by a reconnaissance party, in 157.1 the man is left behind after two years' occupation by the colonists; the man Korobios is not a Theraean in 151.3, the unnamed individual of 157.1 is one of the colonists; in the first passage the spies return eagerly to Thera, in the second the colonists appeal dispiritedly to Delphi. One cannot deny, however, that in accounts with many mythical trimmings repetitions and confusions could occur.

Argument 2 concerns the word Κυράναν, where one would expect Libya. Ferrabino II himself finds it surprising, but points out that it too corresponds to the tradition of Cyrene (Hdt. iv 156.2 and Pindar). This is a point of detailed wording which will be considered later. It need only be said here that it seems very doubtful if this could be said to be distinctively part of the Cyrenaean tradition. Pindar can be discounted; a poet writing when the city had long been established would easily fall into the anachronism, if it is one, whatever the origin of his material. As for the single occurrence of the name Cyrene in Herodotos' Cyrenaean version (156.2) and absence from the Theraean, this seems more likely to be chance than a true difference, especially as the Theraean tradition told of considerable previous information about the area to be colonised (151.2-153). The use of the word Κυράναν may be relevant to the whole question of the document's authenticity, but it seems too flimsy an argument when used to try to prove that the document represents the Cyrenaean, not the Theraean, tradition.

Ferrabino's argument 3 could be significant, however, if admissible. He asserts that the general similarity between the 'pseudo-Theraean' decree of our inscription and that in Herodotos[12] is offset by three fundamental differences in detail. This argument involves discussion of the passage in the ὅρκιον where the text is most difficult to establish.

Oliverio's text (27ff.) runs as follows: ἐπὶ τᾶι ἴσα[ι κ]αὶ τᾶι ὁμοίαι πλὲν κατὰ τὸν οἶκον, υἱὸν δὲ ἕνα, καταλ[έ]γεσθαί τ[ε ἀπὸ τῶγ χώρων ἁπάντων] τοὺς ἡβῶντας, καὶ των [ἄλλ]ων Θηραίων ἐλευθέρος, [ὅ κα λῆι], πλέν. The two important problems are the arrangement of the phrases (or punctuation) and the filling of the one gap of any length. The latest and best discussion of this problem is by Wilhelm,[13] and though uncertainty must remain about the choice of words for the large gap, his ordering and explanation of the phrases seems convincing. He sees that the δέ after υἱόν must mark a division between that phrase and the preceding one (although it need not be adversative), so that Oliverio's καταλ[έ]γεσθαι must govern υἱὸν δὲ ἕνα; a verb needs to be found to govern τοὺς ἡβῶντας, and some qualification for υἱὸν δὲ ἕνα. These considerations led him to propose the following

[12] iv 153. This assumes that the passage represents an actual decree of Thera, as I show to be probable below.
[13] *Griech. Inschr. Rechtlichen Inhalts* (Athens, 1951) 5-7.

text: ἐπὶ τᾶι ἴσα[ι κ]αὶ τᾶι ὁμοίαι πλέν, κατὰ τὸν οἶκον. υἱὸν δὲ ἕνα καταλ[έ]λεσθαι τ[ῶ οἴκω ἑκάστω, πλὲν δὲ] τοὺς ἡβῶντας · κτλ. This text may be used as a basis for discussion and is unlikely to be very far from the sense and wording of the original.[14]

From this text the decree's decision was that one son should sail from each family, that they should be young men, and that of the remaining Theraeans anyone could join the expedition. We may now turn to Ferrabino's three fundamental differences of detail between the ὅρκιον and Herodotos' decree.

He asserts that Herodotos states that the lot was applied in each of the seven villages, whereas in the decree κατὰ τὸν οἶκον. This last phrase should probably not be taken with καταλέγεσθαι, as we have seen, but even if it were there is no difference here, for Ferrabino has misread Herodotos. His words (ἀδελφεόν τε ἀπ' ἀδελφεοῦ πέμπειν) show clearly that we are dealing with families here no less than in the decree. It is after this (τε . . . καὶ) that we come to villages. It is true that Herodotos' words, if pressed, are not exactly || equivalent to the ὅρκιον's υἱὸν δὲ ἕνα. It is generally thought that they mean one brother out of every two.[15] Given Greek ideas about families,[16] it is unlikely that the different wording would often produce a different result, and in fact it seems better to take the words as Herodotos' way of expressing the same provision rather than a fundamental difference. For the general similarity of the provisions is more striking than their difference.

Ferrabino's second difference rests partly on his reading τ[ῶ οἴκω ἑκάστω πάντας] τοὺς ἡβῶντας, which he interprets as meaning a choice

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[14] There still remains the difficulty of exactly interpreting κατὰ τὸν οἶκον. Neither Chamoux's translation (equal rights 'pour chaque famille'; p. 107) nor Oliverio's ("con equali diritti navigare da ciascuna casa;" p. 227) really represent the κατά. Wilhelm's paraphrase (p. 6) is as follows: "Gemäss der Zugehörigkeit eines jeden zu seinem οἶκος, gewissermassen als Vertreter derselben." And to support this historically he adduces the parallel Artist. *Pol.* 1265b, 12ff., where the Corinthian law-giver Pheidon is said to have thought that the number of houses (families) should remain equal. The parallel seems admissible historically, but one wonders if κατὰ τὸν οἶκον can bear so much. Grammatically it must mean either 'by family' (*LSJ* s.v. κατά B) though the plural would be more usual (see examples *ibid.*) or 'for their home' (*LSJ* B "direction towards an object") though this seems redundant, or something like "according to house (family), etc." (*LSJ* iv), which is the way Wilhelm takes it. This last seems best, but it must be noted that the expressions with κατά in this sense are slightly different.

[15] See. e.g. Legrand, 171 n. 1, who would amend to ἀδελφεῶν, thus (incidentally) making Herodotos and the ὅρκιον in agreement.

[16] E.g. Hesiod, *Works and Days* 376.

by age in the families; while Herodotos says that the choice was by lot. In fact Ferrabino's reading makes very difficult sense immediately after "one son per family," and ἡβῶντας can scarcely mean "the youngest" and represent the method of choice. This difference therefore seems inadmissible. The only difference is that Herodotos has mentioned lot while the ὅρκιον does not. This point will merit consideration later, but it is not part of Ferrabino's argument.

Ferrabino says (p. 251) that the third difference is Herodotos' omission of the provision for volunteers. This has no argumentative force. Herodotos' silence could show that he thought such a provision unnecessary to mention; it was probably common to all such enterprises; but it is idle to look for reasons for an omission of this sort, or to argue from it. Thus Ferrabino fails to convince me of his three fundamental differences in detail between the ὅρκιον and the Herodotean decree.

Ferrabino's argument 4 (p. 251) is rendered completely invalid by a proper understanding of the scope of the difference between the Theraean and Cyrenaean traditions, such as I hope to have given above. He says that the provision for return in the ὅρκιον (34-7), though it does not agree exactly with Herodotos' account of the attempted return (156.3), does give this account its "logical presupposition," while the Theraean account does not admit such a pact or its violation. Ferri's explanation of this (p. 23) is that the Theraean account, whether publicly inscribed or simply traditional, avoided every unpleasantness. But the account of the attempted return, if the view expressed above is correct, is not part of the story on which the traditions diverged,[17] and this comparison with Herodotos has therefore no bearing on the question of the origin of the decree.

Ferrabino's fifth argument (p. 251) concerns the dialect of the ὅρκιον. He asserts that it is Cyrenaean not Theraean.[18] The questions of the changes which might be made in republishing a document of another state will be discussed below, and these include dialect. Here I need only anticipate by saying that there are enough

[17] Chamoux, 98, also considers this to be part of the Cyrenaean account. This suggests that he has not noted exactly where the two accounts diverged, in spite of his statement on p. 93.

[18] I have discussed this matter with Miss K. Forbes, who is working on the dialect of Cyrene, and I gather that there are many difficulties in the dialect of the ὅρκιον. But this is not immediately relevant for my purposes here, as I hope to show.

examples to show that the dialect could be altered to that of the publishing state, and so does not in itself determine the origin of the document. In view of this discussion the conclusion must be that Ferrabino's thesis fails; it cannot be said that the ὅρκιον represents exclusively the Cyrenaean tradition.

The second main division of this study, what I would call general considerations, may seem to some extent to float in the air, because it precedes the detailed discussion of the internal evidence, but for purposes of arrangement it is better for it to come first.

In any question of authenticity sound method demands that the burden of proof is with those who argue against it. Hence this paper will mostly consist of an evaluation of arguments against the ὅρκιον's authenticity. Those who have doubted the ὅρκιον's authenticity have assumed either that it was forged for the occasion or that it was taken from previous records, which is simply to date the forgery further back. Apart from the acceptance of the II ὅρκιον's authenticity these two exhaust the possibilities of its origin: if false it is either an epigraphical forgery or the creation of a chronicle, a literary invention. These two possibilities may be examined in turn.

If it was forged for the occasion we must consider its relation to the occasion. The Cyrenaeans decide to give Theraeans full citizen rights in Cyrene according to the ancient agreement. This is apparently one-sided and led Ferrabino (p. 253) to conjecture that the renewal of an old privilege was made by Cyrene in order to make for good relations with the second Athenian League, to which Thera belonged.[19] Zhebelioff,[20] suggesting a later date, would connect it with Alexander's friendly agreement with Cyrene.[21] The choice between these dates cannot be made on purely epigraphical grounds.[22] But it is hard to see why Thera should come into the agreement between Alexander and Cyrene, and Ferrabino's connexion with the second Athenian League is ingenious but far-fetched. It is probably safer to leave conjectures and look at the internal evidence.

The document makes it plain that the giving is all on the Cyrenaean side because the old agreement was felt to be binding. The exact

[19] See *IG* ii² 1.179, *fr.* c. lines 9 and 11, which can be taken to show this.

[20] *C.R. Acad. USSR.* 1929, 429 n. 2.

[21] Diodoros xvii 49.3; Curt. Ruf. iv 7.9.

[22] Mr John Cassels, who kindly examined the stone on my behalf, assures me of this.

connexion of 4f., ἀποδόμεν τοῖς Θηραίοις τὰμ πολιτήιαν κατὰ τὰ πάτρια is with the provision of the ὅρκιον (30ff.) αἰ μὲν δέ κα κατέχ[ων]τι τὰν οἰκισίαν οἱ ἄποικοι, τῶν οἰκείων τὸγ καταπλέον[τα] ὕστερον ἐς Λιβύαν καὶ πολιτήιας καὶ τιμᾶμ πεδέχ[εν] καὶ γᾶς τᾶς ἀδεπότω ἀπολαγχάνεν, but the whole tenor of the document makes it unnecessary to look for further reasons for the Cyrenaean action. If the ὅρκιον had such force for them it seems most unlikely that they regarded it as a forgery for the present occasion.

But they could have been deceived, it might be argued. The *cui bono* argument seems to lead to the Theraeans. The Theraeans could have prepared their forged agreement and had their ambassadors foist it on the Cyrenaeans. There is intrinsic improbability here too; it makes the Cyrenaeans too credulous and careless, and for a great, cosmopolitan Greek city much too ill-informed. Nor does the document look like a forgery for this end: to persuade the Cyrenaeans to give the Theraeans citizen rights. One clause only, that quoted above, can be referred to this object. In the rest there is no concealment of the forced nature of the expedition; the strictly limited right of return to Thera is not hidden (33-7). Even the relevant clause could have been made much more satisfactory for the suggested aim. As it stands, it could easily be contended that it only refers to the first years of the colony; for the document is arranging for the first settlement, and the return provision, next to which it stands, is specifically limited to the first years. An intended deception would surely have provided for a clearly permanent right for all Theraeans to settle in Cyrene. So much for general arguments on the assumption that it is an epigraphic forgery. There remains the approach from analogy.

Epigraphical forgery occurred from time to time throughout Greek history. The earliest we know of is probably that by the Delphian, whose name Herodotos knew but would not tell, who changed the dedication on one of Croesus' gifts to please the Spartans.[23] Theopompus asserted that the Treaty of Callias was a forgery because it was inscribed in the Ionic letters not officially introduced at Athens until the archonship of Euclides.[24] There is no need to list further examples to show that such forgeries are possible. But there is one inscription

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[23] Hdt. i 51.3-4; *cf.* Volkmann in *Convivium, Festgabe für K. Ziegler* (Stuttgart, 1954) 62, "Die Inschriften im Geschichtswerk des Herodot."

[24] *FGH* ii B, 115 F. 154.

analogous to the ὅρκιον, which appears so close that it demands special consideration. This is *Inschr. v. Magnesia* no. 20. Inscribed at Magnesia *c.* 200 BC (*ibid.*, p. 15) it purports to be a decree of all the Cretans conferring benefits on the colonists and || their leader at the time of the colonial expedition. Magnesia on the Meander was certainly founded in the time of the Ionian migrations, and the decree is a flagrant fabrication, as Wilamowitz showed with heavy irony.[25]

Although the difference in the dates[26] perhaps weakens the argument from analogy, it is instructive to compare the ὅρκιον with the Magnesia inscription. The latter is made to bear a superficial resemblance to a foundation decree; there is provision for armed help (20f.) and for money (15ff., 27ff.)—one of the more striking anachronisms. But the goodwill of the Cretans to the colonists is more fully illustrated by their granting them in perpetuity all the interstate privileges of the Hellenistic world (10ff.): kinship and friendship for ever, meals in the prytaneum, the right to import and export without tolls throughout Crete, the right to own property, and, finally, citizenship. Wilamowitz argued convincingly that the motive for this forgery was the desire to avoid the scourge of Cretan piracy,[27] and the document is drawn up so as to convince readers that very friendly relations existed in the past, and to provide antiquity's warrant for privileges in the present.

When the character of *Inschr. v. Magnesia* no. 20 is carefully observed it becomes clear that the differences between it and the ὅρκιον are much more striking than any superficial similarity. The Magnesian document confines itself to specifying privileges and illustrating the great cordiality of the metropolis (the role given to Crete) at the time of the foundation. We have seen that the ὅρκιον is quite different; it concerns itself almost entirely with the necessary arrangements for the establishment of the colony, and there is no attempt to show any cordiality on the part of the mother city. The Magnesia inscription shows how a forgery of this kind was made to gain present political advantage. The fact that the ὅρκιον is not like it suggests that it does not belong to the same family.

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[25] Konon (*FGH* i 26) F. 1, no. 29. The tradition is fully discussed by Wilamowitz, *Hermes* xxx (1895) 177ff. See also *Inschr. v. Magnesia* p. 16.

[26] The ὅρκιον was inscribed at Cyrene *c.* 250 years after its presumed date; *Inschr. v. Magnesia* no. 20 some 800 years afterwards.

[27] *Hermes* xxx (1895) 191.

But even if it is difficult to take the ὅρκιον as a forgery for the occasion, there remains our second possibility: that it was taken from a chronicle of earlier date, and is an earlier literary invention. Inscriptions based on literary sources of a historical kind are found quite frequently. The Parian Marble, which refers to historians, was inscribed in the first half of the third century BC, and is only the most famous of many. Other examples of "inscriptions of a literary character" are collected by Larfeld,[28] and Forsdyke discusses some of the most notable in a chapter of his *Greece before Homer* (1956) called 'Public Instruction'. These tend to be chronicles or lists of priests and magistrates and are not obviously comparable to the ὅρκιον. They are also of a rather later date, the third century BC and after. At the beginning of the second century BC the inscriptions *Inschr. v. Priene* nos. 37 and 38 explicitly quote historians in connexion with an international dispute,[29] but the character and date of our document, which claims to be an ancient decree, seem to make the analogy too distant. *Inschr. v. Magnesia* no. 17 is an account of the foundation of Magnesia on the Meander, and so is at least close in subject-matter. It is a typical *ktisis*,[30] decked out with forged Delphic oracles,[31] and was considered to be a complete fabrication by Wilamowitz,[32] though Schmid believes we have a "bestimmte überlieferte Tradition."[33] But being a chronicle with the stock components of the developed *ktisis* of Hellenistic times it is by no means analogous to the ὅρκιον. A true analogy of a decree copied from an earlier literary invention escapes me, but we can still argue the matter on the assumption that this is the ὅρκιον's origin.

The direct literary source is not Herodotos; and, as far as I know, no scholar has || suggested that Herodotos is the direct source of our document. That he is not is clear from the details in the ὅρκιον which are not in Herodotos' account, though the question of the relationship of Herodotos and the ὅρκιον will have to be mentioned later. The Herodotean account is a very full one, but the ὅρκιον has more detail on the actual official act of sending out the colony. If

[28] *Griech. Epigraphik* 510.

[29] See *FGH* iii B, 491 and Commentary.

[30] It is used as a good example of a 'Ktisissage' by P. B. Schmid in *Studien zu griech. Ktisissagen* (Diss. Freiburg i.d. Schweiz, 1947) 94ff.

[31] See Parke and Wormell, *A History of the Delphic Oracle* (Oxford, 1956) i 54.

[32] Pp. 190ff.

[33] See n. 30 above.

a literary invention, therefore, it must be an invention of an author more interested in Theraean affairs[34] even than Herodotos. Thus it seems improbable that this would be an author of a general Greek history like Hellanikos, for all his industry. The possible sources would seem to be a local chronicle or a *ktisis*, a foundation story. The latter seems attractive, but should probably be dismissed, for the early poetical *ktiseis* are extremely shadowy; the literary form is only certain and common in Hellenistic times.[35] The local chronicle remains and this brings us to the question of date. The question can be put in this form, was it before or after Herodotos?[36] Those who think the ὅρκιον and Herodotos have a common source[37] clearly choose the former, so we may look first at the possibility of a pre-Herodotean local chronicle.

Jacoby believes that local history comes after what he calls great history;[38] Herodotos did not use local historians so much as produce them. It seems rather overbold for a modern, however well we know the fragments, to dismiss the famous statements of Dionysios of Halikarnassos (*de Thuc.* 5.819) about the predecessors of Herodotos and Thucydides. These certainly suggest some quantity of historiography, even if it is denied this title, and Dionysios had access to all the material. For all that, Jacoby's thesis only appears wrong if pressed to the point of denying any local chronicles before Herodotos. In general he has made the point that local history follows 'great history'. Thera was not a large or flourishing place by the fifth century, so that on a necessarily risky argument from probability it seems unlikely that the few early local historians would include one of Thera. There is thus some improbability in the belief that Herodotos and our document had a common source in a pre-Herodotean chronicle of Thera.[39] It is worth noting that, even if this chronicle had

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[34] Or possibly Cyrenaean, though it is slightly against probability and certainly against the principle of economy of explanation to imagine that the Cyrenaeans had a chronicle which the Theraeans exploited.

[35] See Jacoby, *Atthis* (Oxford, 1949) 364 n. 62. Ion's and Hellanikos' prose *ktiseis* seem to be the earliest definite examples.

[36] Whether or not one accepts Jacoby's conclusions about early Greek historiography (see n. 38 below), he is clearly right to look at all other early writers in their relationship to Herodotos.

[37] See n. 6 above.

[38] E.g. *Atthis* 68, 289 n. 111.

[39] As Wilamowitz and Chamoux, see n. 6 above.

existed, on the evidence of Dionysios' statement we should be chary of taking the author for a creator of fictions. For he states that these early writers had one aim: to present for the knowledge of all as many memorials as they found preserved in cities and tribal states, in temples and in archives, not adding or taking away anything themselves.

If there are difficulties in the way of assuming a pre-Herodotean local chronicle of Thera, a post-Herodotean one is less satisfactory as a source for the ὅρκιον. The date of the invention comes close to the inscription's publication, so that this suggestion becomes vulnerable to the same objections which were raised against the idea of a forgery for the occasion. The invention would also, one would expect, reveal the dominating influence of Herodotos, and I hope to show later that the ὅρκιον's relationship to Herodotos does not seem to be that of copy and model.

These arguments all show that considerable difficulties beset the theory that the ὅρκιον is a copy of a literary invention of earlier date. We have seen that there are also difficulties in regarding it as a forgery for the occasion. The general questions I want to ask are therefore concluded by this one: are the difficulties in taking the ὅρκιον as genuine greater than the ones we have found in taking it as false? If it is genuine it should suit seventh-century Thera, and this must be investigated next.

Some of the matters relevant to this question belong to the examination of the details || of the text, but the general considerations are plain enough and sufficient for the immediate purpose. The document reveals a Thera politically organised, with a monarchy and an assembly,[40] capable of arranging a colonial expedition, of conscripting citizens for it, of inflicting the death penalty and confiscating property for the state, and with a fully developed sense of citizenship.[41]

As we have seen no precise or definite analogies of the right date exist. But there are one or two from an early enough period to be of some use. The famous inscription from Dreros[42] is of the right

[40] This in particular will require further discussion in detail, but for the moment we are testing the document as it stands against the probable conditions of seventh-century Thera.

[41] Clearly shown by the provision about return, 33ff.

[42] See *BCH* lxi (1937) 334ff., with the correction in *BCH* lxii (1938) 194f. Published in Buck, *Greek Dialects* (Chicago, 1955) no. 116.

date[43] and shows, as Ehrenberg has emphasised,[44] a developed polis organisation, some specialisation of magistrates and the concept of the polis. For the polis is defended against the individual. Thera is not far from Crete, which flourished particularly in this early period. In spite of the controversy about its date the Spartan Rhetra may also be admitted as a parallel, and this shows not only a well-developed constitution and concept of the state, but also the co-existence of monarchy and assembly in a Dorian community.[45] The constitutional stone from Chios should perhaps be dated rather lower than Wilamowitz's 'um 600'; but this even shows definite democratic arrangements, such as *rhetrai*, an assembly and the council of the demos.[46]

These are not more than general analogies on the constitutional development of Greek states, but when added to the probable situation of Thera in the seventh century they gain in value. In his little paper "die archäische Kultur der Insel Thera" (Berlin, 1897), based largely on archaeological remains, Hiller von Gaertringen described the archaic period as its 'Glanzzeit' (p. 8), and noted in particular its very early type of alphabet (p. 14f.). This suggests that writing was introduced to Thera early and its use is shown to have been widespread certainly by 600 by the earliest epitaphs and inscriptions from the gymnasium.[47] Though no state document is included in the inscriptions from this early period, the familiarity with inscriptions shows that the seventh-century Theraeans could well have made a written record of a state act. At a similar date the people of Dreros did and the Spartans had already made a record of the Great Rhetra. The fact that inscriptions before 500 BC are comparatively rare tends to make moderns think that public documents also must have been rare before that date. Chamoux, for example, shows this ten-

[43] Guarducci dated it to the beginning of the sixth or possibly end of the seventh century, *Riv. di Fil.* xvii (1939) 20.

[44] *CQ* xxxvii (1943) 14-18.

[45] Plut. *Lycurgus* 6. Although his own date seems extremely high Hammond showed the weakness of the argument for a low date (sixth century) in *JHS* lxx (1950) 42ff. (see p. 42 n. 3 for bibliography). Andrewes, *The Greek Tyrants* (London, 1956) 73, argues sanely for a date in the seventh century. It certainly cannot be later than Tyrtaios.

[46] Tod, 1; Wilamowitz, *Nordionische Steine* (*Abh. Berl. Akad.* 1909) 64ff. Arguments for a date between 570 and 550 are advanced by L. H. Jeffery, *BSA* li (1956) 156ff. For the contents *cf.* Tod's commentary p. 2.

[47] *IG* xii 3.563ff. and 762ff.

dency, when he writes that the decree claims to be from too ancient a period to be authentic (p. 110). But, as has been pointed out, though inscriptions are originals to us, they were in fact published copies of a few originals chosen from all those on more perishable material in the state archives.[48] The greater number of inscriptions of post-archaic date reveals an increased tendency to make public and permanent records; it does not reveal the non-existence of state documents in the early period. In general, therefore, the analogies and the probable situation of seventh-century Thera do not suggest that either the ideas or the existence of our document need be dismissed as anachronistic.

We may now turn to more detailed examination. The title of the document[49] 'ὅρκιον || τῶν οἰκιστήρων' as usually translated 'oath of the founders'[50] might be nisleading. There is no oath recorded as such, and it might be argued that the compilers knew there should be an oath, but had no record of one. As it stands our ὅρκιον is clearly not an oath. This argument against the decree's authenticity is removed, however, by a different translation, 'the agreement of the founders'. This makes better sense, is perfectly in accord with usage (for ὅρκιον means solemn agreement as early as Homer, see *LSJ* s.v.) and can be justified from the inscription itself. In lines 8f., 18, 40, 47, 49 ὅρκιον is used, and in all of these the translation 'agreement' is perfectly possible, if not preferable; in 14f., where ὁ̣ι̣ώρκωσαν makes it certain that an oath is in question, the word used is ὅρκον.

The first words after the title, ἔδοξε τᾶι ἐκκλησίαι, raise several questions. Probably the two most important are whether this sanction formula would be posssible in a seventh-century document and whether the constitutional idea behind it is anachronistic. But others need consideration: if it is derivative, a product of the fourth century, then what was its origin? Is there any significance in these particular words in contrast to the more regular ἔδοξε τῷ δήμῳ or τῇ βουλῇ καὶ τῷ δήμῳ? The more important questions will be more easily discussed after these last ones.

At Thera there was an ἐκλησία [*sic*] in the fourth century (*IG* xii 3 Supplement 1289.5) though there is no instance of its appearance

[48] Well stressed by Klaffenbach, *Griech. Epigraphik* (Göttingen, 1957) 50f.

[49] That it is a title and not the bare record of an oath (like the frequent Attic ὅρκος) seems certain from the larger-sized letters and 18f.

[50] E.g. Chamoux, 107; Oliverio, 226.

in an introductory formula. Nor indeed have we any evidence for the formula introducing Theraean decrees till the third century, when we have [δε]δόχθαι τῶι [δ]άμωι (*IG* xii 3 Supplement 1290.10) and ἔδοξε τᾶι βουλᾶ[ι] [καὶ τῶι] δάμωι (1291.2f.). What is the value of this slight evidence? There was an assembly called an ἐκλησία at Thera before the triumph of the *koine*, and, in late examples, the formula ἔδοξε τᾶι ἐκκλησίαι does not occur. Little though this is we can say that the sanction formula of the ὅρκιον was probably not copied from contemporary Thera.

But we must also attempt to discover if the formula could have come from any other fourth-century source. The first possibility must be Cyrene itself, but we possess only one piece of relevant evidence, the beginning of the contemporary decree preceding the ὅρκιον. Here the formula is δεδόχθαι τῶι δάμωι (11). This is strong evidence, even though it stands alone, that the formula of the ὅρκιον was not taken over from current practice at Cyrene.

The only examples of relevant date known to me of the words ἔδοξε τᾶι ἐκκλησίαι standing alone as a sanction formula[51] are from Calymna, Epirus, Leros and Oropos.[52] It is scarcely likely that the composers of the document went to any of these states for models, so that we may answer our first question by saying that the evidence does not show that the formula was copied from a contemporary source. It is, however, true that the only examples of the formula that we now have are of the fourth century or later.

The next question, whether these particular words are significant, is made necessary by our knowledge that one important decree regulating colonial affairs was passed by the 'assembly for sacred matters'.[53] But it must be admitted that the comparison of examples of this sanction formula with the more normal ones reveals no significant differences. It seems to have been simply an occasional alternative.[54]

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[51] I exclude such phrases as ἁλιαίαι ἔ[δοξε τελεί]αι: Argos, *IG* iv 557.2.

[52] See 'Tituli Calymnii' by M. Segre, *AS Atene*, xxii-xxiii (1952) 1.A.1f., B. 11ff.; 2.1f.; Collitz-Bechtel, *Sammlung der griech. Dialektinschriften* 1335.6, 5520.4; *IG* vii 4250.2f., 4251.2f. All these are, more or less confidently, assigned to the fourth century.

[53] Tod, 33.44f.; but see the republication by W. Vollgraff, *Verhand. d.k. Nederl. Akad. van Wettenschappen* (*Letterkunde*), Nieuwe Reeks li, 1948, no. 2. For the assembly see *fr.* vi 44f. (p. 9).

[54] In addition to those in n. 52 I list the following examples: *IG* xiv 256.7, 612.1, 952.10; these, from the Western Greeks, are not quite parallel as the formula does not stand alone; it is, with slight variations, ἔδοξε τᾶι ἁλίαι καθὰ καὶ τᾶι βουλᾶι.

We may thus conclude that the II sanction formula was not invented according to current practice at Cyrene or Thera, and it was not used because of its inherent suitability to such a document. But we note that it is only paralleled from documents of the fourth century or later.

Wilamowitz said that ἐκκλησία is 'unglaublich in dem alten Thera' and that for ἔδοξε ἆδε would have been written.[55] The word ἐκκλησία and this use of ἔδοξε do not occur before the fifth century, so the argument from silence lies at hand.[56] To argue definitely from the non-appearance of a word before a certain date is to ignore the chance nature of our record of Greek words, so we must look also at analogous material. In early or primitive decrees the sanction formula tends to be either brief or non-existent,[57] as on the early Elian bronzes from Olympia.[58] But the decree from Dreros begins with a definite sanction formula similar in nature to those of later documents: ?Αδ' ἔϝαδε πόλι. As the first editors stated, analogous Cretan documents suggest that πόλι is equivalent to τοῖς Δρηρίοις and shows that the assembly of citizens is responsible for the decision.[59] An assembly capable of state decisions has already been shown to be conceivable in seventh-century Thera. The Drerian document suggests

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The same phrase is restored at line 6 of no. 13 of *Asylieurkunden aus Kos*, Herzog and Klaffenbach., *Abh. deutsch. Akad.* (Berlin, 1952) no. 1, and the restoration is defended on p. 24 by reference to *IG* xiv 256.7. But the phrases are not exactly parallel, so there is room for doubt. Also, from Corcyra, we find ἔδοξε τᾶι ἁλίαι in *IG* ix. I 685.1, 686.1, 687.1, 688.1. All these are honorary decrees of the third century or later. The earlier ones in n. 52 are also of this nature. In the case of the Western cities, Corcyra and Leros, we have no comparative evidence, but Calymna provides a large number of similar decrees with different sanction formulas. Nos. 3, 18, 25, 42, 43, 44, 47 have ἔδοξε τᾶι βουλᾶι καὶ τᾶι ἐκκληδίαι; the majority have ἔδοξε τᾶι βουλᾶι καὶ τῶι δάμωι. Here the formula clearly has no distinguishing significance. At Oropos we find, apart from the two mentioned in n. 52 above, two with ἔδοξεν τῇ βουλῇ καὶ τῇ ἐκκλησίᾳ (*IG* vii 4256.5f., 4257.5f.). These are dated to the end of the fourth century by Dittenberger (notes to 4256). From a slightly later period onwards there is a long series of similar proxeny decrees, *IG* vii 236ff., which use the more normal formula. Here we have only a change of fashion, for the two which use ἔδοξε τᾶι ἐκκλησίει are a little earlier (see Dittenberger, notes to *IG* vii 4250).

[55] Pp. 38ff. (see n. 4 above).

[56] Though Professor Andrewes has drawn my attention to the ἔκκλητοι at Sparta (see Wade-Gery, *Essays in Greek History*, Oxford, 1958, 190), whose existence and name might suggest the possibility of an early assembly called ἐκκλησία at Dorian Thera.

[57] *Cf.* Meyer, *Forsch. z. alten Geschichte* i 295f.

[58] Buck, 61ff.

[59] See *BCH* lxi (1937) 342.

that such an assembly could head a document with a formula expressing its sanction. On the other hand the words ἔδοξε and ἐκκλησία seem unlikely to have been used. Thus the answer to the two most important questions about these two words is this: evidence and probability suggest that the actual words of the sanction formula are not original, though the existence of a formula with this meaning would not be a constitutional anachronism.

The word αὐτομάτιξεν (25) raises the question of words in the text of purely lexicographical or dialect interest. As a matter of general principle I do not intend here to draw attention to such words unless they seem relevant to our main question. For example, ἀποι[κίξαι] in line 26[60] would be, if of the seventh century, far the earliest appearance of ἀποικίζω. But the word is regularly used by Herodotos and Thucydides meaning to colonise, and the fact that it does not occur before Aeschylus (*fr.* 304, *OCT*, Sidgwick) is most probably simply due to the chances of survival. The bulk of writing preserved from before 500 BC is comparatively slight, yet the language had been called upon to express much of human and political experience long before this. In the case of a simple active verb meaning to colonise the first record of the word could well post-date its first use by a long interval. But there remain a few which are significant for our purposes, although they raise for the most part only lexicographical questions, and these may be taken together here.

αὐτοματίζω is a regular late word for to prophesy, the earliest recorded use apart from our inscription being Diodoros xvi 92.2 (see *LSJ*, Add. et Corr. 2055). Contemporary (i.e. fourth-century) uses of the word which have been recorded are quite different in meaning (*LSJ* s.v.). On the other hand it is clearly wrong to say that this is a late word (in this sense) and deny the possibility of its authenticity. But for the one occurrence in our inscription it would be stated that the word is confined to the first century BC and later; the fourth-century occurrence shows that it is much older, though it does not show how much older. But it is true that it is an unusual word and χράω would be expected in its place.

πολιτήια (32) meaning rights of citizenship or the like is not found before Herodotos, from whose writings onwards it is frequent (see

[60] The restoration is not certain; see Ferri, *Historia* iii (1929) 392.

LSJ s.v.). The original meaning is as here. Although it would be regarded as a 'recent' word on this evidence, it is a word which, one would imagine, would quickly follow the use of πολίτης (πολιάτας as in line 36 or πολιήτης). This is found as the antonym of ξένος from Alkaios onwards (*LSJ* s.v.). The concept is clearly not out of place in the seventh century, so there is perhaps nothing particularly suspect in the appearance of the word here in spite of the lack of other pre-Herodotean instances.

ἀδεσπότω (33) meaning 'unoccupied' is not exactly paralleled until Strabo (xvii 797). In the fourth century the word normally used in such circumstances may have been ἀδιαίρετος (undivided). Later settlers at Black Corcyra were to have a share of τᾶς ἀδιαιρέτου (*Syll*³. 141.10). The first record of the word ἀδέσποτος is in the high-flown language of a Platonic myth (*Rep.* 617e) where ἀρετή is called ἀδέσποτον. The adjective would quickly arise from the noun however, and although δεσπότης is not attested before Pindar and Aeschylus (*LSJ* s.v.) the compilers write (*ibid.*) "not in Homer (for metrical reasons) though he uses δέσποινα." It is difficult to find an obvious archaic word to expect in this sense. In describing the Cyclops' island Homer uses the adjectives ἄσπαρτος, ἀνήροτος (*Od.* ix 122), but the composers of a decree would need more legal, less poetic words. ἄκληρος means without owner in the Homeric *Hymn to Aphrodite* 123, ἄδαστος is Sophocles' word (*Ajax* 54). In a Herodotean oracle (iv 159) ἀναδαιομένας means 'distributed'. This variety and the lack of an obvious alternative should make one chary of condemning ἀδέσποτος, which gives the exact sense required.

ἀδιέως (36) only occurs here (*LSJ* Add. et Corr.) though the adjective ἀδεής is frequent from Homer onwards. It is only worth mentioning here because of the occurrence of ἀδιής = ἀδεής in one of the archaic rock-cut inscriptions at Thera (*IG* xii.3 552), a connexion noted by Wilamowitz. The connexion does not, however, prove anything about the origin of the decree, especially as it could be simply chance.

θανάσιμος (37) meaning 'liable to the death penalty' is a natural extension of the word's basic meaning 'belonging to death, pertaining to death', and it is use here is not classified separately in *LSJ*. But the only other use of the word precisely parallel to this legal meaning is in the Cyrenaean constitutional document, *SEG* ix 1.53. There the offence of changing Ptolemy's arrangements in Cyrene

is punishable by death, and the expression used is θανάσιμος ἔσται αὐτός. This document is dated at the earliest to 322-321 BC,[61] so is later than *SEC* ix 3. It is possible that its wording was influenced by that of the ὅρκιον, with the more common ἔσται replacing the ὅρκιον's τένται, a dialect form (see *LSJ* s.v. τέλομαι). On the other hand it could well be argued that the phrase was a regular Cyrenaean one of the fourth century, and its use in the ὅρκιον shows the Cyrenaean origin of this phrase. The meaning is not sufficiently unusual for any definite conclusions about origin to be drawn from the use of θανάσιμος here, but the existence of the exact Cyrenaean parallel certainly suggests a Cyrenaean origin.

The remaining detailed points for discussion involve more than purely lexicographical matters. It has been suggested that the presence of the name Βάττος (25f. and 27) shows that the document cannot be from seventh-century Thera,[62] and if Herodotos' statement (iv 155.2) about the name Battos II is accepted it must be conceded that the name could not have appeared in the original document. Herodotos suggests that the name Battos originated in the Libyan name for king, which it was, and Pindar knows another name for the founder of Cyrene, Aristoteles (*Pyth.* v 87). Then the story of Battos' stuttering becomes an aetiological tale to explain the foreign name. Herodotos' explanation is accepted by Parke, for example,[63] who considers that no oracle containing Battos' name is authentic; for him Pindar's retention of the other name is sufficient confirmation of Herodotos' conjecture. But there are difficulties, which have been discussed recently by Legrand and Chamoux.

Legrand objects that if the name was a title meaning king all the kings should have borne it.[64] In fact it alternates with Arcesilas like any other Greek name. If Battos had a former name it is surprising that neither Herodotos' Theraean nor his Cyrenaean informants apparently knew it; just as it is surprising that Herodotos did not know what Pindar did.[65] Both French authors[66] note that Battos is

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[61] *Cf.* Ehrenberg in *RE* xvi 1100; Taeger, *Hermes* lxiv (1929) 453ff.
[62] See Hiller in *RE* s.v. 'Thera' 2292f.
[63] Parke and Wormell, 74f.
[64] P. 155 (n. 10 above).
[65] Since the former name is so relevant to the argument Herodotos puts forward, the *argumenta ex silentio* seem admissible. Chamoux's (p. 97) "cet autre nom qu' Hérodote ne deigne pas préciser" seems to gloss over the difficulty.
[66] Legrand, 155 n.; Chamoux, 95.

a perfectly acceptable Greek name[67] connected with the word 'to stutter', like the French Lebègue, although most of its holders do not in fact stutter. Legrand suggests that the stammering legend, which would easily grow up aetiologically from the name and is not flattering to the ancestry of the Battiads, comes from the anti-Battiad Cyrenaean source,[68] while the explanation by connexion with the Libyan word for king was put about by a pro-Battiad source which noted the similarity of the Libyan word. Possibly the family itself had made the connexion in order to improve their standing with the natives, and Delphi was glad to agree as it showed the oracle's prescience and command of foreign tongues. Chamoux, without referring to Legrand, makes similar suggestions: either the name Battos gave rise to the stammering legend and the resemblance to the Libyan word for king was coincidental, or the title was taken to impress the natives and then became a proper name. Legrand's explanations seem most plausible, but for our immediate purposes we need only note that Herodotos' suggestion is open to doubt, and thus the presence of the name Battos in our document is not necessarily to be regarded as a later intrusion.

As noted above the occurrence of the name Κυράναν in line 25 has also been regarded as a sign of falsity. The name is probably from the pre-Greek 'Aegean' language, as its suffix -ήνη shows, and the Greeks are supposed to have found Cyrene inhabited by a Libyan population,[69] so that it is not impossible that the Theraeans knew of the place and its name before sending out the colony. Previous Greek knowledge of the area is certain. The other true foundation decrees we possess, though of course of much later date, name the actual city to be founded,[70] On the other hand, Herodotos' account shows that the first Greek attempt was made on the island of Platea (iv 153, 156.3), so that either Platea or the more general Libya would seem more likely to be authentic.

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[67] It is a common enough Greek name (*cf.* Pape, *Griech. Eigennamen*³, 1911, s.v.). To his list add *IG* vii 1556; *SEG* i 352.1, ix 174, 175, 176, xii 237.12. The earliest examples, however, are the kings of Cyrene, and it is possible that all the others were derived from them. The earliest non-Cyrenaean bearer is the Corinthian leader in Thuc. iv 43.6.

[68] P. 156.

[69] See on this whole question Bertholdy, *Mélanges Boisacq* i (Brussels, 1937) 47ff.

[70] Naupaktos, Tod, 24; Brea, Tod, 44.

A further point to be noticed in the opening of the decree is the form of the first sentence. In it we are given the motivation of the act in a clause beginning with ἐπεί before the Theraean decision itself, expressed in the words ὁριστὸν δοκεῖ Θη[ραίο]ις. Such an arrangement of the opening is characteristic all over the Greek world in the fourth century, but is not found to my knowledge in early inscriptions,[71] so that it would seem very unlikely to belong to an authentic seventh-century inscription. ||

Of the phrase ὁριστὸν δοκεῖ Θηραίοις Wilamowitz wrote "ganz neu; ὁρίζω im solchen Sinne ist sonst wesentlich poëtisch." On the whole the appearance of a rare word suggests authenticity rather than the opposite. It has been seen that a poetic source is not likely, but it could still be argued that the word was inserted to give the forged document a high-flown appearance. The matter seems too uncertain for any clear conclusion to be drawn.

The appearance of the word ἀρχαγέταν (27) was quickly seen to be particularly interesting,[72] for one of the ancient inscriptions from Thera (*IG* xii 3.762) contains this word, and it can be argued that the Theraeans used it for king.[73] Wilamowitz maintained that it was the 'echt' word, and the addition of Βασιλῆα could be taken as an explanation of it and considered an interpolation. He has been followed by others.[74] Certainly the connexion with the language of seventh-century Thera is encouraging, but objections can be made to the suggestion that the two titles or offices are simply two words for the same thing, one old, one new. In the first place there is the fact that Herodotos also has two words in the passage iv 153, in which he records the Theraean decision to send the colony: εἶναι δὲ σφέων καὶ ἡγεμόνα καὶ βασιλέα Βάττον. The implications of this will be discussed below, but, as Herodotos could not have copied our document, his use of two titles makes one disinclined to see βασιλῆα as a Cyrenaean explanation of ἀρχαγέταν. Secondly, there was a cult of Battos Archagetas at Cyrene (*SEG* ix 72.22) as also of Apollo

[71] See Larfeld, *Griech. Epigraphik* 343f. His statement that the form of opening with the motivation expressed by an ἐπεί or ἐπειδή clause is found in early documents was supported by his first example only, and this is now differently restored and dated to the fourth century (see Hiller, *IG* v 2 xix 65ff.).

[72] See Wilamowitz, 40.

[73] See the discussion by Hiller, *JdI* xlvii (1932) 127ff., where he defends the reading Ἀρχαγέτας Προκλῆς, i.e. King Prokles, in preference to taking Ἀρχαγέτας as a proper name. By connecting it with King Prokles of Epidauros and arguing from letter forms he comes to the date first half of the seventh century.

[74] Besides Hiller (p. 129) see Risch, *Mus. Helv.* xi (1954) 34 n. 59.

Archagetas (e.g. our own inscription, line 10). The Cyrenaeans therefore were accustomed to the word in its more familiar sense of leader, especially colonial leader (see *LSJ* s.v.). Thus the two titles express two aspects of Battos' position, as could be expected; he both led the expedition and was king of the new city.

The phrase ἐπὶ τᾶι ἴσα[ι κ]αὶ τᾶι ὁμοίαι in 27f. raises the questions whether this equality is in place in a colonial expedition of the seventh century and whether this formula would have been used to express it. From the middle of the fifth century the phrase is frequently found, a widely used formula (*LSJ* s.v. ἴσος). The earliest occurrences are at Gortyn (*Inscr. Cret.* iv 72.2), in the Athenian decree about Hestiaea (*IG* i^2 40-41.4) and in Thucydides (i 27.1). The Gortyn inscription is of the same period as the great Law of Gortyn and to be dated *c.* 450 BC,[75] and that from Hestiaea not long after 446 (*loc. cit.*). All three deal with land or colonial settlement. The appearance at Gortyn suggests that the phrase was an old-established formula for such uses rather than a recent Attic invention. It is noteworthy that neither of the two important words are the products of democracy. The Spartan peers were ὅμοιοι, and ἰσονομία was an aristocratic ideal in sixth-century Athens.[76] Although there is to my knowledge no evidence from early colonies which shows that lots were equal,[77] it certainly was the normal behaviour later, even in oligarchic communities,[78] and the Greek obsession with γῆς ἀναδασμός suggests a familiarity with the idea of equal shares in the land, which colonial practice may well have provided. For these reasons there seems to me no need to be suspicious of this provision.

The word ἑταίρους in 27 suggests authenticity. We know that there were *hetaireiai* at both Cyrene (see our inscription, 16) and Thera (*IG* xii.3 450.18). Clearly the group came from Thera to Cyrene. Such groups grew out of the Homeric ἑταῖροι,[79] the band of close companions of a lord, and it is presumably in this sense that the word is used here: || the colonists are the ἑταῖροι, of Battos. There is perhaps also the possibility that the meaning is "those Theraeans who

[75] See Guarducci, *Riv. di Fil.* xvi (1938) 264ff.

[76] See Ehrenberg, *Aspects of the Ancient World* (Oxford, 1946) 89, discussing the Harmodios scolium.

[77] The story of Aithiops, the Syracusan colonist mentioned by Archilochos (see Athen. 167d), merely shows that the colonists all had lots, as we should expect; it does not say that they were equal.

[78] E.g. the Corinthians, Thuc. *loc. cit.*

[79] See Busolt, *Griech. Staatskunde* i 250f., 327f.

are members of the *hetaireiai* shall sail," but the wording seems to favour the other alternative. In either case the unusual expression, when combined with the existence of the *hetaireiai*, has an authentic look.

One of the most striking aspects of the document is the change from regular infinitives after δοκεῖ to narrative aorists at line 40. While Ferri (p. 22) decided from this that the document is genuine to this point, but that the narrative section is based on a historical source, Wilamowitz, using most doubtful etymological arguments, thought that the narrative section was ancient because it is free from 'recent' words, and the decree forged. Chamoux too (p. 109) thinks that this section proves the decree to be forged. But these authors have not expressed all the implications in this intrusion of narrative.

The most important one concerns the question of forgery. If the whole document was forged for the occasion, then it seems certain that it would all have been in the normal decree style and the infinitives would have persisted throughout. There would have been no difficulty in expressing what we are told in narrative in terms of decreed instructions. Thus the distinction between the narrative section and the decree proper seems to show that the whole document is not a forgery for the occasion; to append narrative to a forged decree is not a way to convince people that it is an archaic Theraean enactment. If there was no attempt to deceive, the narrative, by its very distinction from the rest, also shows that the composers knew exactly the scope of the decree; they thus had a source in which the decree existed as a decree.

This investigation of the text itself has revealed several words or phrases which are probably of the fourth century (ἔδοξε, ἐκκλησίαι, θανάσιμος), and though arguments can be found in each case against concluding this certainly, to employ these arguments in such a number of instances (and others may have escaped me) would suggest special pleading. On the other hand the general arguments against the hypothesis of forgery are reinforced by the appearance of ἑταίρους for example, and by the implications of the added piece of narrative. If some of the wording probably belongs to the fourth century but the document in general seems genuine, then the evidence points to the conclusion that we have a genuine document edited in the fourth century.

Although there is no precise parallel it is worth looking at the changes which Greeks admitted in publishing texts. The first question is that of changes of dialect. If we do not include treaties among

them, the only decrees regularly published outside the issuing state were honorary decrees; hence this is the only category of decree considered by Buck in his treatment of this question.[80] These, he finds, were normally published in the dialect of the state responsible for the decree.[81] Our document is, however, not an honorary decree, so that it is probably wrong to apply this rule and conclude that it should be in Theraean. The Athenian Coinage Decree (*ATL* D.14) is probably a better analogy. The examples of this which have been found show that this Attic decree was published in some places with Ionic spelling and forms, for example in the fragments from Cos and Aphytis, and even with some simple changes of wording. If our document is placed in Buck's category of treaties, as it perhaps should be, it would be normal for it to be in the dialect of the publishing state. Thus it would not be at all abnormal or surprising if the Cyrenaean publishers changed a Theraean document into their own dialect.

But the changes seem likely to have gone beyond dialect. It must be remembered that the Greeks did not demand complete verbal accuracy as we do in legal documents. A revealing example is found in the Quadruple Alliance of 420, of which we have two texts, one on stone (Tod, 72) one in Thucydides (v 47). Thucydides purports to give the exact terms, yet there are changes, which Tod lists (p. 177). These are orthographic, certain changes of form not affecting the sense, a few omissions, and in one case an addition. || Although attempts have been made to account for these, even by changing Thucydides' text to force agreement, a remedy rightly rejected by Buck (p. 156 n.), the truth is probably that Thucydides, even when copying, allowed his own usage to replace the wording of the text, omitted a few words thought inessential, and even added on one occasion words he thought necessary.

Purely epigraphical evidence shows the same thing. Wilhelm showed that the two copies of the Attic decree about first fruits at Eleusis (Tod, 74) were very probably cut by the same stone-cutter, yet variations between the texts occur, and this when the two communities concerned were as closely connected as Athens and Eleusis.[82] Similarly

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[80] *AJP* viii (1913) 133ff.
[81] A notable example is *IG* xii.21 5.
[82] *ÖJh* vi (1903) 10ff., especially 12 and 14.

quite considerable differences are found between Athenian and Delphic editions of the same Amphictyonic Decrees.[83]

If such differences were allowed between contemporaneous copies, we should not be surprised if changes were made in publishing a very ancient Theraean text at Cyrene. Such a text would appear very different from the decrees they were used to. Its opening especially would seem bare, and it is around the opening that the wording seems particularly suspect, as we have seen. One remembers Polybios' difficulties in interpreting the ancient treaties between Rome and Carthage (iii 22.3). The Great Rhetra is again an illuminating parallel; as Hammond has pointed out, the actual wording cannot be original, as it is not in the Laconian or Delphic dialect.[84] Analogies have shown how far the Greeks were ready to change the wording of documents. A seventh-century Theraean decree would be hard to understand and unfamiliar in form for a fourth-century Cyrenaean. Some editing seems only too probable, and the fourth-century appearance of some of the wording, and especially of the opening, could well be attributed to this.

In conclusion there is one further matter relevant to this discussion which demands consideration; it is the passage Hdt. iv 153, the sentence beginning Θηραίοισι δὲ ἕαδε. It raises the question whether Herodotos or his source knew of a Theraean decree about the colony, and, if so, whether it was our document in its ancient form. The lines of inquiry available are Herodotos' practice about recording such documents and the comparison of the contents of this sentence and our decree.

So far as I know the state documents buried in Herodotos have not received the treatment given to those in Thucydides. The inscriptions in Herodotos have been well discussed by Volkmann,[85] but these do not include possible treaties and decrees. Thucydides, though he occasionally quotes treaties, never gives us the exact words of decree. When he gives what was clearly a decree of the people his practice is to give his own paraphrase, just as Herodotos does, as can be seen by comparing, for example, Thuc. vi 8.2 and Hdt. v 97.3. Even when he gives many clauses of what was almost certainly a recorded decision, as in viii 67.3 (*cf.* 69.1), the establishment of

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[83] These are listed by Colin, *BCH* xxiv (1900) 89.

[84] *JHS* lxx (1950) 44.

[85] See n. 23 above.

the Four Hundred in 411, he uses his own words and is, we may be sure, much briefer than the legal phrases of the original.

There are many treaties and public decrees in Herodotos' work. The treaties are listed by von Scala, *Die Staatsverträge des Altertums* (1898), though he omits i 150.2. I add here what appear to me to be the probable Greek decrees in his work: i 151.3, 152.2, iii 46.2, iv 146.2, 153, v 96.2, 97.3, vi 22.1, 66.1, 106.3, vii 144.3, ix 106.3. Although he never reveals his source it is difficult to believe that none of these was taken from written record. He never quotes, but simply introduces the main decision in his own words with ἔαδε or ἔδοξε. The passage iv 153 is one of the fullest records of a public decree; detailed enough to make one reasonably sure he is using a written record, yet so brief as to suggest he has only given what he wanted. There is no possibility of proof or of showing his source, but it can be said that the comparison with other state decisions recorded in his work shows that he is probably giving here a definite Theraean decree. ||

If we compare Herodotos' decree with the ὅρκιον we note that Herodotos only gives the decision to send out the colony, the choice of leader and the choice of personnel. These topics are all relevant to his immediate description of the despatch of the colony; the other matters dealt with by the ὅρκιον would be less relevant. Close comparison is only possible between the arrangements for personnel and the choice of leader. We have already noticed that both Herodotos and the ὅρκιον give Battos two titles or offices; Herodotos uses ἡγεμόνα and the ὅρκιον ἀρχαγέταν for the first; both use βασιλεύς for the second. The concept of both, that Battos is to lead the expedition and be king of the new state, is clearly the same. This agreement is striking.

In view of the textual uncertainty it is difficult to draw any certain conclusions from comparing Herodotos' arrangements for personnel with those of the ὅρκιον. We have already seen that his ἀδελφεόν τε ἀπ' ἀδελφεοῦ, though different from the ὅρκιον's υἱὸν δὲ ἕνα, is probably only another way of expressing the same provision. His addition of πάλῳ λαχόντα, and his statement that the men came from all seven villages could be looked on as explanations about Thera and procedure necessary for a general history. The latter phrase is certainly different from the decree's provision for volunteers, though it could be an extension in thought to the effect of that provision. The fact that Herodotos' decree is much shorter and

yet contains certainly one statement not found in our document (the choice by lot) makes it most improbable that our text was made up from Herodotos by filling out his bare bones. For such a process everything in his brief sentence would certainly be utilised before recourse to invention. Comparison between the ὅρκιον and Herodotos' decree thus reveals on the one hand that it is wrong to regard the ὅρκιον as a forgery based on Herodotos. On the other hand the exact agreement on the two titles of Battos and on the provision of personnel in the most important point (i.e. conscription by families) shows that it is not impossible that Herodotos or his source had access to the ancient Theraean decree. Certainly the passage is favourable to the hypothesis of a recorded decree at Thera and more favourable than not to the possibility that this is the decree produced with changes of wording at Cyrene.

This attempt at a full consideration of the arguments bearing on the question of the authenticity of the ὅρκιον τῶν οἰκιστήρων has not led to a proved conclusion. The difficulties in general in assuming the document to be a forgery, either of the fourth century or earlier, have been shown, yet some of the words and expressions have been seen to be probably not original. The matter seems to come to a question of wording; can a document which is not in the exact words of the original be called authentic? We should not think so. But we ought not to let our preoccupation with exactly authentic wording lead us to condemn the document as a whole as false. For this rigid attitude conflicts with other evidence and strong arguments. Unless these can be clearly disposed of it seems better to regard the content of the ὅρκιον τῶν οἰκιστήρων as original. ||

7. THE DATE OF THE GREEK PENETRATION OF THE BLACK SEA

Introduction

In the Western Mediterranean, and particularly in Sicily and Italy, our knowledge of Greek colonization has benefited very significantly from archaeological discoveries. In particular the archaeological finds have been combined with the literary tradition about dates of foundation to provide a firm and generally accepted chronology of the colonial movement. In the East, however, in the Pontus and Propontis, where there has been less archaeological exploration, our reconstructions have perforce a less secure basis. In particular the date when the Greeks first entered the Pontus remains uncertain, and the arguments which are used to establish it are all dangerous or unsatisfactory. We meet many old friends or enemies: the *argumentum e silentio* in the literary evidence, in the archaeological its counterpart, the argument from absence of finds. To these may be added arguments from probability and those from uncertain analogy. Yet this is not a reason for despair; only a reason for us to keep our aims within the limits the evidence imposes, and not to try to draw conclusions more precise or more sure than it allows.

The general picture from the literary evidence is that our earliest Greek writings, the Iliad and the Odyssey, make no explicit reference to the Pontus. This was noticed in antiquity. Strabo writes (VII, 298) But neither Homer nor the others were acquainted with distant parts. There are some forty rivers flowing into the Pontus, yet he does not mention one, even of the most famous ones, like the Danube, Tanais, Borysthenes etc. . . . And this is natural, for the sea was as yet unsailed, and was called "Ἄξενος owing to its bad weather and the savagery of the people who lived around it". Beside Homer's apparent ignorance may be set the tradition of the colonial foundation dates. These are very conveniently set out in a table by R. M. Cook, in his article *Ionia and Greece 800-600* BC (*JHS* LXVI, 1946, p. 77). We find that the great majority of dated colonies in the Pontus and Propontis belong to the seventh century. Astacus (711) and Parium (709) in the Propontis are attributed to the end

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of the eighth, but much more || notable exceptions are Cyzicus in the Propontis, the first foundation of which is dated 756, and Sinope and Trapezus in the Black Sea. The latter is also dated 756, and as its mother-city was Sinope, Sinope is by implication given a still earlier date. The foundations are thus mostly of the seventh century with three mid-eighth century dates standing in sharp contrast.

The archaeological finds were also reviewed by Cook in the paper mentioned and he was able to state that nothing Greek had been found of an earlier date than the second half of the seventh century. To my knowledge this is still true of the Pontus, though earlier material has recently been found in the Propontid region, which will be discussed later on. In the main the archaeological evidence clearly supports the literary tradition of a predominantly seventh-century colonization of the region.

Good reasons have long been known why the Greeks should have turned to other areas before settling in the Pontus. The Pontus has a different climate from the Aegean, and Myres stressed well that colonists seek if possible a climate like that of their homeland (cf. *CAH* III, 633). It was also a forbidding sea for Aegean mariners, without the many islands of the Aegean, and with a bad reputation for storms. Add to this the barrier of the Bosporus, with its strong prevailing southward current, and the lesser one of the Hellespont, and a predominantly seventh-century colonization is easily intelligible.

Thus we need have no doubt that the archaeological and literary evidence, supporting each other as we saw, reflect the true picture, and the matter would perhaps not require further discussion, had not the attempt been made to go beyond this acceptable conclusion, to a more extreme one: that the Greeks did not enter the Pontus at all till the seventh century. The most thoroughgoing and original exposition of this view is an article by Rhys Carpenter in *AJA* LVII 1948, pp. 1-10. In this his main contention is that the Greeks could not penetrate the Black Sea until they had invented fast rowing ships capable of rowing up against the currents in the Straits. A further reason for a discussion of the question now is that there has been recent Turkish excavation at Sinope and in the region of Cyzicus, both sites, as we have seen, dated exceptionally early in the literary tradition. It is thus my intention to examine these new arguments which have been brought to bear on this question, and the scheme of this paper is as follows: first Rhys Carpenter's thesis and the nav-

igation of the Bosporus will be considered; next we shall look at the archaeological evidence to see the exact conclusions to be drawn from it; finally other evidence bearing on the question, largely from literary sources, will be brought in, and this will include the myths.

The Navigation of the Bosporus[1]

Rhys Carpenter's starting point is the absence of archaeological finds in the Pontus earlier than the seventh century. He combines this with the literary foundation dates, which shew that the main colonization of the Pontus was later than e.g. in the West, and concludes that the Black Sea was closed to Greek sailors before *c.* 700 BC.

This date is chosen because Thucydides tells us (I, 13, 3) of improved shipbuilding techniques at Corinth at that time. What Thucydides actually says is that the Corinthians were the first of the Greeks to have triremes, and Ameinocles ll of Corinth made four ships for the Samians about three hundred years before the end of the war he is describing. The normal interpretation is that Ameinocles was the first Greek to build triremes *ca.* 700 BC. This passage has raised much discussion, which need not be considered in detail here as it is not germane to our present purpose. The chief difficulty is that we also know from Thucydides (I, 14, 1, 2) that penteconters were the normal warships until shortly before the Persian Wars, when the leading navies first equipped themselves largely with triremes. Rhys Carpenter firmly concludes from this (p. 8) that Ameinocles' invention was of the penteconter. His argument here involves the difficult question of Homeric ships as well as the rejection of Thucydides' exact words, which makes it at least uncertain, but there is no need to challenge this single point when the validity of the whole argument is to be questioned.

This argument depends on the nature of the current in the Bosporus. The surface current flows generally in the direction from the Pontus into the Propontis. The speed of the current reaches a normal

[1] I should like to express my gratitude for advice to Mr. Philip Ullyott of Robert College, Istanbul, an expert on the hydrography and navigation of the Bosporus; to Mr. Arthur Lee, hydrographer of the Ministry of Agriculture and Fisheries; and to London University and Bedford College for grants from their research funds, which helped me to visit the area under discussion.

maximum in the narrows towards the southern end of three to four knots. In the summer, the ancient sailing season, the prevailing wind is from the North, contrary for a ship trying to enter the Black Sea. Rhys Carpenter therefore concluded that the Greek sailing ship could not have made its way against this adverse current. It was necessary to have rowing power of some four knots, which, he says, the new rowing ships provided.

In a somewhat circular fashion he then says it follows that no colonies in the Black Sea can have been founded before 700 and all myths localized there must be more recent than this date. In fact he attempts greater precision still, and chooses *ca.* 680 as the date when the first Greek ships rowed through the previously impassable straits, for Chalcedon was founded about then, and Chalcedon, he says, was clearly chosen as a good starting-place for the voyage (p. 9).

There are various weaknesses in this argument; for example the uncertainty about Ameinocles' invention, which we have noted, and perhaps too great reliance on absence of archaeological finds of a given date. But the two most important weak points are the statements about the effect on navigation of the currents and winds in the Bosporus, and the suggestion that the Greeks could only take rowing ships into the Black Sea.

These two points are justly attacked by B. W. Labaree in his article *How the Greeks sailed into the Black Sea* (*AJA* LXI, 1957, pp. 29-33). His main point is that penteconters are not suitable for regular trade, and we know that the Greeks did pursue a regular trade with the Black Sea countries. A penteconter with a crew of fifty to pay and built for speed not for carrying cargo would have been quite unsuitable for the corn trade which we know was so important in classical and later times. This trade happens to be an aspect of economic history well illustrated by the literary evidence, notably from the orators, who shew that it was normal, regular and considerable.[2] There is also an explicit reference to ships sailing up against the current in Dionysius Byzantius' *Anaplus Bospori*, which we shall be consider-

[2] The trade, as it was in the fourth century, is well described in Roztovtzeff's *Social and Economic History of the Hellenistic World* I pp. 105-111. See Demosthenes XXXV, πρὸς τὴν Δακρίτου παραγράφην, which is entirely concerned with money lent for the Pontic trade; there are also clear references in other speeches which shew how normal and how considerable the trade was, e.g. L, 6 and 17, XX, 31ff., Isoc. Trap. 57. The same point is clearly revealed in the quaint treatise on Economics called Ps. Arist. Oeconomica II; see the edition by B. A. Van Groningen, Leyden

ing fully later. It is thus quite clear that sailing ships regularly sailed through the Bosporus into the Black Sea, and Labaree is right in stressing this point particularly, since it alone demolishes Rhys Carpenter's argument.

Labaree does not however complete the task of criticizing Rhys Carpenter on this question of penteconters. For to support his contention that penteconters were used for trade and perhaps first developed for that purpose Rhys Carpenter refers to the passage Hdt. I, 163, 2 where Herodotus states that the Phocaeans made || their great voyages of discovery in the Western Mediterranean in penteconters not in merchant vessels. Rhys Carpenter says that it was the penteconter's "greater cruising power" which made the Phocaean voyages possible. This is difficult to accept. The Mediterranean round ship, the sailing merchantman, had all the cruising power necessary for Mediterranean voyages throughout antiquity; it sailed night and day, while the penteconter, needing fifty men's rations, fifty men's energy, would normally beach for the night for food and sleep. Compare Thuc. II, 97, 1 and Hdt. IV, 86, 1 where distances are reckoned by the day and night sailing of a merchantman. Thucydides makes the ship cover at least 115 nautical miles in the 24 hours, Herodotus 130. The trireme could in "a very long day" cover 120 nautical miles (see Xen. Anab. VI, 4, 2) and the trireme will clearly have been considerably faster than a penteconter.[3] In the famous sprint of the second trireme from Athens to Mytilene in 427 Thucydides specially notes that the men ate on board and took turns to sleep, shewing that this was exceptional (III, 49, 3).

Herodotus' information about the Phocaean voyages is normally understood in a different sense, surely correctly. His stress that the Phocaeans used penteconters not round ships shews that this was exceptional. The Phocaeans used them, it is usually thought,[4] to combat piratical attacks in the Western Mediterranean. We remember the reputation of the Etruscans, whose name Τυρρηνός became

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1933, Pt. II, 3c, 10. The treatise dates from the period 325-305 BC (see *op. cit.* pp. 40ff.). A much earlier reference is Herodotus' description of the corn ships seen passing through the Hellespont by Xerxes (VII, 147, 2f.).

[3] For these calculations of speeds and distances from the ancient evidence see *Companion to Greek Studies* para 542.

[4] See How and Wells, *Commentary ad loc.* and Torr, *Ancient Ships*, Cambridge, 1894, p. 5 n.

almost equivalent to pirate.[5] Possibly the Phocaeans were ready to indulge in piracy themselves, as the Samians did with their penteconters under Polycrates (see Hdt. III, 39, 3) and as Dionysius the Phocaean did after the failure of the Ionian Revolt (Hdt. VI, 17).

We may thus conclude that penteconters were not used for regular trade. There was regular trade with the Pontus. Therefore the Greeks did sail up the Bosporus. This is the simple answer to Rhys Carpenter: "you may calculate that they could not sail up the Bosporus, but the evidence shews that they did." For intellectual satisfaction, however, we also want to know how they could, why in fact Rhys Carpenter's calculations are wrong. This too was attempted by Labaree but here his work is less satisfying.

He attacks particularly Rhys Carpenter's point that sailing up was impossible because the prevailing winds in the sailing season are contrary, and tries to demonstrate that favourable winds blow sufficiently to allow the passage of the Bosporus by sailing ships. For this he uses the published wind tables for the region. The information about currents, winds, and navigation is given in the *Black Sea Pilot* published by the Admiralty, which reached its tenth edition in 1955. The information in the seventh edition of 1920 was repeated in the *U.S. Hydrographic Office Black Sea Pilot* of 1926, and it is from this that Labaree takes his figures. This is unfortunate, as the eighth edition of 1930 introduced much more sensitive wind tables, based on better meteorological observations, which are repeated in the later editions. The passages about the hydrography of the Bosporus were revised in the light of the latest research in the 9th edition of 1942, and substantially repeated in the latest edition. Rhys Carpenter used the ninth edition, and thus had at his disposal the latest material on the subject, so that it is surprising and retrograde of Labaree not to use at least as up-to-date information as the author of the article he attacks.

Favourable winds for sailing up the Bosporus are the southerlies. From the wind tables of the 1920 *Pilot* Labaree took the number of days in each month on which winds blew from a southerly direction and called these days favourable || for sailing up the Bosporus. But, because he assumed that in conditions of exceptionally strong

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[5] See H. A. Ormerod, *Piracy in the Ancient World*, Liverpool 1924, pp. 152ff. for the ancient references.

current it would be difficult to sail up even with a favourable wind, he deducted from the number of favourable days a certain percentage, calculated on the following considerations. Since the flow of water from the Black Sea into the Aegean is finally produced by the outflow of the great Russian rivers in particular into the Black Sea, and that flow is hastened by northerly winds at their mouths, Labaree concludes that the current in the Bosporus will be particularly strong after northerly winds on the North coast of the Black Sea. Wind tables are provided in the *Pilot* for Sebastopol, Odessa and Varna (though, if he had used the latest *Pilot*, Labaree would not have failed to notice that this last town is now called Stalin). Taking the mean figure of northerly winds recorded in each month at these three ports, Labaree subtracts that figure from his favourable days for the month at Istanbul and so achieves three figures; first days of ideal conditions, when winds over the Bosporus and the Northern Black Sea are southerly; second those when the Bosporus has southerly winds but the Black Sea northerly; these he calls 'probable' sailing conditions; thirdly 'impossible,' when the winds of both Bosporus and Northern Black Sea are northerly. To give one example, in August, the worst month, 2.6 days are ideal, 2.4 days are probable, and 26 are impossible.[6]

These calculations are vitiated not only by the inferior wind tables used but also by ignorance of the conditions affecting the rate of flow of the Bosporus current. The more accurate and sensitive wind tables of the 1930 and later *Pilots* do not simply give the number of days of say southwesterly winds in a month, but take account of the diurnal variations in winds in the area by giving the percentage of observations taken at 7.00 a.m. and 1.00 p.m. of the various winds in any one month.

As to the conditions affecting the current, our knowledge of these depends on a basic paper recording German measurements made in 1917 and 1918.[7] The measurements make it clear that the decisive influence in determining the rate of flow of the surface current is exerted by the winds over the Bosporus itself. There is no suggestion that winds in the Black Sea have any appreciable effect. Northerly winds over the Bosporus increase the speed of the flow,

[6] See p. 32 for these figures and calculations.

[7] See L. Moller and A. Merz, *Untersuchungen in Bosporus und Dardanelles, Veröffentlichungen des Instituts für Meereskunde*, Heft 18, 1928, pp. 223-30.

southerly winds hold it up. The conclusion therefore follows that favourable winds for sailing up at Istanbul will also bring favourable current conditions.

To obtain a rough picture of the proportion of time favourable for sailing up the Bosporus in the months of the sailing season, we need only look at the percentage of southerly winds at Istanbul. The percentage of southerlies observed at 7.00 a.m. is; April 32, May 25, June 16, July 7, August 7, September 13; and at 1.00 p.m.; April 27, May 33, June 24, July 12, August 11, September 16. The difficulty about the winds for sailing ships seems to be removed by these figures.

On the other hand the Bosporus has always presented a difficulty to navigators, and Rhys Carpenter's work is useful in reminding us of this. Even to-day large modern steamers are careful to avoid the main force of the current as far as possible, so that we can imagine it was a formidable obstacle to an ancient round ship. It seems worth considering briefly the nature of the current and methods of navigating the strait open to ancient shipmasters.

The following facts come from the *Black Sea Pilot*, 10th edition 1955, pp. 22-4 and 28-31. In normal conditions the main surface current sets southerly, reaching in the Southern part of the strait a speed of 3 to 4 knots. In very exceptional || circumstances seven knots have been measured off Roumeli Hisar, at the narrowest point, the so-called Devil's Current. Also in very exceptional wind conditions, with southwesterly gales, the current can be held and even reversed, so that it flows north into the Black Sea; nevertheless the prevailing wind blows in the same direction as the current.

The main current chooses the shortest route from point to point, and there are always counter-currents or back-eddies flowing northwards up the sides of the bays. They frequently flow at half a knot or more, and they are strongest when the main current is strongest, when it is weak they tend to die away. There are two points at which the main current is described in the *Pilot* as filling the whole width of the strait, namely at Therapia and Karibjeh. Both these are towards the northern end, where the current is comparatively slow. At Karibjeh the main part of the current attains half to one knot, at Therapia it flows at more than one knot only in the worst winds. In the rest of the strait there are always eddies at the shore of greater or lesser extent and strength, according to place and weather conditions, unless the main current itself disappears.

These counter-currents are used, I am assured by Mr. Philip Ullyott, by all ships to-day, not only small boats, but even large steamers as far as they can. I learnt from the same source that small boats without engines regularly get up the Bosporus to-day, and the sailors will sometimes tow their boat from the land if the current is too much for them. This, the behaviour of such modern boats which regularly make the passage, is the natural analogy for us to use in conjecturing how the ancients sailed up the strait.

There is a further device employed by the local sailors to-day, the use of the so-called *kanal*. This is an underwater counter-current which flows from the Sea of Marmora up the Bosporus into the Black Sea (see *Pilot* p. 21). At the southern end of the strait where the surface current is strongest the *kanal* is approximately thirty to sixty feet below the surface. It is utilized as an assistance for boats sailing up the Bosporus by lowering something like a sea anchor into it and so being drawn along by it. Although there is no ancient reference to this, as far as I know, it would probably be quickly discovered by fishermen by the behaviour of their nets, and so it might be suggested that this device too could have helped ancient navigators. But this clearly cannot be pressed.

One reason why I should be reluctant to press it is that we have one book from antiquity which tells us something about the navigation of the Bosporus. It was not used by Rhys Carpenter or Labaree yet it saves us from depending completely on modern analogies, however cogent they may be. The *Anaplus Bospori* by Dionysius Byzantius[8] was probably written in the latter part of the second century AD,[9] so it is not from the period in which we are at the moment interested. But sailing merchant ships did not alter greatly throughout antiquity, and Dionysius' remarks on the navigation of the strait will have been true for the earlier period.

The author shews considerable interest in the sea; he describes good harbours and good fisheries, and is particularly interested in the behaviour of the current. This he describes carefully and accurately in his general introduction pp. 1 ll.8-11, and 3 ll.1-8, and draws attention to very fast rough places ll and the like. He also mentions navigation in several passages, of which two, which seem particularly relevant, deserve quotation.

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[8] See the edition by R. Güngerich, Berlin, 1927.

[9] *Op. cit.* pp. XLIIIf.

The first (p. 21, ll.15ff.) refers to a place now called Arnaoutköy (Ἑστίαι in Greek), which is at a narrow point towards the southern end. He writes "And I have seen many loaded ships, running before the wind with their sails set, borne backwards as the force of the current fought against the wind. But it turns round again, shaving the rock, and owing to the back eddy is drawn on against the sea, running by itself as if pulled in the opposite direction. Fear overtakes the sailors and they would despair of everything if they had not an alternative course. Thus mostly they tie ropes to the ships and tow them from the shore forcibly against the current as it pours against it with great opposing force; but sometimes when the current slackens a little they struggle against the stream at the very edge of the landing place, just by the rocks, using the rocks as supports for their oars and strengthening their power on sea by help from the land. Certain tracks and impressions of sea-going crabs are shewn passing by the place of strongest current." The second passage concerns the narrowest part at Roumeli Hisar and runs "After the temple of Diana Dictynna the ship's path is turbulent and rough owing to the strictly confined stream; the place is called Pyrrhias Cyon, in my opinion from the similarity the sea has here to a dog, but the majority state that it comes from a shepherd's dog which runs around the shore barking at those who have to steer close to the shore because of the violence of the current. Here indeed the strait is most narrow, dividing the two continents."

These two passages, the first of which refers expressly to ships proceeding by sail (though ready to help themselves with oars), while the second shews us ships hugging the shore to avoid the adverse current and obtain the help of the counter-current, clearly justify our analogies from modern practice, as we should expect. On the other hand Dionysius' interest in navigation tempts me to apply the *argumentum e silentio* to the question whether Greek sailors used the underwater *kanal*. His interest in navigation, it could be argued, must have led him to mention it, if it had been utilized in this striking way.

But this is a small and uncertain point. It is sufficiently clear that Rhys Carpenter's thesis cannot stand. The Greek sailing ship could navigate the Bosporus during the sailing season, and we cannot date the Greek penetration of the Pontus by technical improvements in the rowing ship.

The Archaeological Evidence

As mentioned earlier no Greek archaeological material earlier than *ca.* 650 BC has been found in the Pontus. When R. M. Cook wrote the article cited above, in which this point is stressed (see pp. 76 and 82), the sites archaeologically explored were Istros, Olbia with Berezan, and Apollonia Pontica. Although the archaeological dates are slightly lower than those in the literary tradition (see Cook's table p. 77), this tradition gives seventh-century dates to these colonies, so that the absence of earlier archaeological material is not remarkable, and does not affect the eighth-century dates attributed to other sites. Those dates are to be tested by exploration of the sites to which they belong, hence the importance of excavations during the last few years at Sinope and near Cyzicus. ||

Some twenty miles to the south of Cyzicus Professor E. Akurgal has excavated on the site of a considerable city. His results up to that year are recorded in *Anatolia* I 1956, pp. 15ff. They shew an entirely Greek settlement (p. 24) with pottery going back to *ca.* 700. Akurgal identifies the city as the ancient Daskyleion, capital of the third Persian satrapy, and his reasons seem strong. But this is not relevant to our present enquiry. We are interested in its earlier significance. If there was a new Greek settlement of considerable size at about 700 twenty miles inland from Cyzicus, we must conclude that by 700 the Greeks on the coast were sufficiently strong and numerous to colonize well in the interior.

The excavations of Akurgal and Budde at Sinope are recorded in *Vorläufiger Bericht über die Ausgrabungen in Sinope*, Ankara 1956. On pp. 4ff. the oldest pottery found is discussed and shewn to date from the end of the seventh century. Akurgal concludes from this evidence that the foundation of Sinope falls in the last quarter of the seventh century, like that of the other Pontic sites which have been archaeologically explored.[10] He cites Rhys Carpenter's article approvingly on the date question.

The true conclusion for chronology from the earliest finds on a site is that they provide the archaeological *terminus ante quem.* To proceed further and say that the earliest finds provide the date of the site's first occupation it is necessary to be sure that the whole site

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[10] This view is put more abruptly in his short report for *Anatolian Studies*, V, 1955, p. 23.

of the earliest settlement has been completely explored. In many instances sites of cities were moved in antiquity,[11] so that one cannot always be certain that the oldest settlement has been discovered. Thus Akurgal's conclusion must be looked at in the light of the topography of Sinope and the extent of his excavation.

Modern Sinope lies on the neck of a small peninsula stretching Eastwards into the Black Sea. The town begins just beyond the narrowest part of the sandy neck connecting it with the mainland and widens out as it stretches up the peninsula some little distance. The description of Polybius (IV, 56, 5ff.) shews that the ancient city of his day had a similar position. The great value of the site is the harbour on its south side. It is the one harbour on the south coast of the Pontus truly protected from the North wind. When I was there, in September, there were heavy waves continually battering the north wall, while the sea was completely calm in the south harbour. The mediaeval walls together with the sea on two sides provide the modern boundaries except to the East where the modern town spreads a little beyond then. These walls are not only full of ancient material, but in both the West wall at the isthmus and the East wall there are sections of undisturbed ancient walling of the finest quality, perhaps of the fourth century or a little later. This means that Hellenistic Sinope was exactly on the modern site, and its walls stood where the walls stand to-day.

Because of the existence of the modern town there has been no excavation of any extent within the walls so that we are left to conjecture if the city stood on its present site before the walls were built which partly remain *in situ*. But our conjecture on this occasion may be confident. Colonial sites on an island just off the coast or a peninsula are common.[12] They gave the colonists a point on the very edge of the land largely defended by sea. The attraction of its protected south harbour at the isthmus and these analogies from other sites put it beyond reasonable doubt that the pre-fourth-century city was also on the modern site. ||

This means that although we know where the most ancient settlement probably was, that settlement is protected by the modern town

[11] Smyrna is a clear example, see Strabo XIV, 634.

[12] Cf., for example, Berezan, see E. H. Minns *Scythians and Greeks* pp. 451ff.; Iasos, see *Ath. Mitt.* XV 1895, p. 139 and plan plate III; Cyzicus, see F. W. Hasluck, *Cyzicus*, Cambridge 1910, pp. 1ff. and plans in back cover.

and inviolate to excavators. Thus Akurgal naturally looked for the cemetery. He dug in the sand dunes on the mainland just across the isthmus, and it was there that he found the tombs containing the pottery mentioned. This stretch of ground is the nearest land outside the modern city of no agricultural value, and it is therefore used as a cemetery to-day. For the same reason we may suggest, though we cannot be sure, that it would have been chosen as a cemetery from the beginning of the city's existence. The character of the land, however, and its use as a cemetery throughout the life of the city, must make it uncertain whether the oldest graves have necessarily been preserved. For the ground is shifting sand blown about by the winds from the sea, and where graves of the 20th century AD. lie adjacent with those of the seventh century BC one can be sure that many of the early graves have been disturbed. So we have the position that the earliest settlement has not been found or excavated and the cemetery is of such a character that it would be hazardous to assume that examples of graves from all periods have been preserved. It is clear therefore that the earliest pottery found can only provide the archaeological *terminus ante quem.*

In the Pontus the archaeological position has not been changed by the excavation at Sinope. It is true that one of the sites with an eighth-century foundation date has been partially explored and yielded nothing earlier than the end of the seventh, but the nature of the site and the excavation do not allow the firm conclusion that the eighth-century date is to be rejected. The excavation at Daskyleion has shewn that the Greeks were firmly established in the Propontis by *ca.* 700, but the application of this to the question of the entry to the Pontus is not perfectly simple. Rhys Carpenter, it is true, coupled Propontis with Pontus, because the Hellespont presented a similar, though lass formidable, barrier to the Bosporus, but from the point of view of colonization Propontis and Pontus are very different. In climate as Myres pointed out (*CAH* III p. 658) the Propontis is a little Aegean; we have already noted that the Pontus was different and superficially unattractive. They need not, therefore, be regarded as one territory colonially or archaeologically. We shall need to return to the question of the connection between the two seas. For the moment the conclusion about the Black Sea from the archaeological evidence must remain unchanged: we have archaeological evidence of Greek penetration from *ca.* 650 BC.

Other Evidence

Other evidence which might be used to take us beyond this conclusion from the archaeological evidence is almost entirely from literary sources. This literary evidence is either direct or indirect. The direct evidence, that is to say definite statements that the Greeks were in the Black Sea by a certain date, is confined to the literary tradition about the dates of colonial foundations. The rest may be called indirect, for it depends on our inferences, as for example from Homer's silence, and our interpretations of mythical material.

It is time to look a little more closely at our direct evidence: the high foundation date stated for Trapezus and implied for Sinope. There is no need to linger over Trapezus; Eusebius simply gives the date 756. That of Sinope is more complicated. The literary tradition states definitely that Trapezus is a colony of Sinope,[13] so that if both these statements about Trapezus be accepted, || the foundation of Sinope must be earlier than 756. The date for Sinope in Eusebius however is 631, so that there appears to be a conflict here. This conflict may be resolved by reference to the very full account of the early founders of Sinope given in Ps.-Scymnus.[14] After mythical founders—the Amazons and opponents of the Amazons—and Syrians came Habrondas, a Milesian by race. These Greeks were driven out by the Cimmerians, and the place was re-established by exiles from Miletus, whose names the author also gives. As Bilabel noted in his careful discussion of the passage (see *Die ionische Kolonisation*, Philologus Suppl. XIV 1920 pp. 30ff.) the full and detailed account suggests a good source or sources, and it is clearly easy to combine with the information from Eusebius. The Eusebian date, 631, is of Ps.-Scymnus' last foundation, a refoundation made necessary by the Cimmerian invasion, and the date inferred from that of Trapezus would be applied to the pre-Cimmerian foundation by Habrondas.

There are several points of interest in the account of Ps.-Scymnus, such as the prosopography and the possible sources, but it is not necessary to go into these here, where my purpose is only to shew

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[13] For list of sources see *P/W* 2. Reihe. VI, 2. col. 2216.

[14] ll.941-952 in Müller *Geographici Graeci Minores* I, but 986ff. in Diller, *The Tradition of the Minor Greek Geographers*, 1952, who sensibly suggests that the anonymous author would be better named *auctor ad Nicomedem Regem* (see p. 42); but see also p. 177 where it is conjectured that Pausanias of Damascus (Const. Porph. *De. Them.* I 2, p. 18. 3-8) is the name of the author.

that we have a clear literary tradition of an eighth-century foundation. A sentence of Herodotus has been interpreted as denying a pre-Cimmerian settlement (e.g. by Bilabel p. 32, Cook p. 72); it runs (IV. 12, 2): "the Cimmerians seem to have fled from the Scythians into Asia, and to have settled on the peninsula where the Greek city Sinope now stands." These words do not exclude the possibility of a Greek settlement at some time before the Cimmerian invasion, and so the plain statement of such a settlement in Ps.-Scymnus should not be rejected simply on a doubtful inference from Herodotus.[15] The literary tradition thus supplies clearly two mid-eighth-century foundation dates in the Pontus. These high foundation dates for Sinope and Trapezus have often been doubted,[16] but a view of their worth can only be taken after all the other evidence has been considered. They remain the only direct evidence for an eighth-century date for the Greek entry of the Pontus.

Our indirect evidence may begin with an interesting inference from an archaeological find. In the excavations of Old Smyrna briefly described by J. M. Cook in *Illustrated London News* 28 Feb. 1953 (p. 329) there was found a signed vase of *ca.* 650 BC. bearing the name Istrokles. Cook says that this name shews knowledge of the Danube, the Ister or Istros in Greek, and since the Milesian potter

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[15] The chief reason why Bilabel interprets Herodotus' sentence as refuting Ps.-Scymnus' account can be seen in his emphasizing Herodotus' κτίσαντες by printing it with the letters widely spaced. Clearly he thinks κτίζω must mean "colonize for the first time." But the word is also used by Herodotus to mean colonize or settle generally without implication about earlier settlement; see I, 16, 2 where Smyrna is called ἡ ἀπὸ Κολοφῶνος κτισθεῖσα, although Herodotus knew that Smyrna was first colonized by Aeolians (I, 149f.). His other reason for doubting the pre-Cimmerian settlement lies in the word δοκεῖ in the line about Habrondas (1.993): ὑπὸ Κιμμερίων οὗτος ⟨δ'⟩ ἀναιρεῖσθαι δοκεῖ. Bilabel says this shews that the author or his source had no certain evidence but used personal conjecture. This may be so, though we must remember the exigencies of metre. It may reveal that the evidence spoke of an earlier settlement than that of 631, and Ps.-Scymnus (or his source) combined this with the information about Cimmerian occupation in Herodotus in the way a modern historian might. It may also be that Ps.-Scymnus (or source) did not know if Habrondas himself had been driven out by the Cimmerians and so uses δοκεῖ. It is possible that Habrondas was, even if he founded Sinope before 756, for we know from Assyrian records that the latest date for the Cimmerians' appearance in the Caucasus area is 705 (see *P/W* s.v. Kimmerier col. 400). But all that is really clear is that Ps.-Scymnus had records of a pre-Cimmerian Greek settlement of Sinope. The full list of sources for Sinope's foundation may be found in Bilabel *loc. cit.*, or more concisely in Magie, *Roman Rule in Asia Minor* II, p. 1074 n. 18; Ps.-Scymnus' account is the fullest.

[16] E.g. by Cook *op. cit.*, Meyer, *G.d.A.* I p. 433, Beloch, *Gr. Ges.* 1, 2, p. 233.

cannot have been born after *ca.* 670 the region of the Danube must have been known and the Pontus entered by the early seventh century at the latest. It is likely that this rare name, not attested elsewhere, would arise most easily from the knowledge of the mighty river, but the existence of a town in Crete and a small island off Cnidus both called Istros (Steph. Byz. s.v.) must make Cook's argument not quite certain.

The same river occurs in an attempt to date the entry of the Pontus through Hesiod. In Theogony 1.339, in a famous enumeration of rivers, we meet the Ister. By the time this passage was written there is thus no doubt that the Greeks knew the Danube. The date when the passage was written cannot be certainly established, but it is normally conjectured by the rivers which the poet mentions. Since it is a long list and clearly intended to include the greatest rivers of the world it is thought significant that no Western river is mentioned, the Po's absence being especially notable.[17] The argument that no Greek poet later than 700 could have been ignorant of the Western world is clearly strong in view of the eighth-century colonization of Sicily and Italy. It would be reasonable therefore to say that this line shews that the Pontus was known to the Greeks by at the latest 700 BC. ||

A similar line of argument would seem applicable to two fragments of Eumelus of Corinth. One (17 in Kinkel, *Epicorum Graecorum Fragmenta*) is his statement that there were three Muses, daughters of Apollo, the third of whom he calls Borysthenis. The second (K. 8) is his opinion that the nymph Sinope was daughter of Asopos. As the traditional date of Eumelus is well in the eighth century, on the face of it these fragments would suggest that a famous river and city of the Pontus were known to a Corinthian poet of the eighth century BC. But it is not enough to look simply at the surface impression, for the poet's date and the attributions of the fragments to him are not beyond dispute.

The evidence on the poet and his works is cited and discussed at length by Will (E. Will, *Korinthiaka*, Paris 1955, pp. 124ff.). Will puts forward the suggestion that the name Eumelus may conceal several different poets of different ages. He stresses that of the mere five fragments preserved in verse four are Homeric in diction, but the

[17] See, e.g. T. W. Allen, *Homer, the origins and the transmission*, London 1924, ch. 24.

fifth (K. 13) approaches the language of choral lyric with Dorisms and imported Aeolisms. These are the lines for a men's choir of Messenians at Delos, the only poem Pausanias considered genuine (Paus. IV. 4, 1). Will thinks this point of language makes it hard to identify the poet Eumelus, author of the Korinthiaka, with the author of this poem. It is found 'seltsam' by Schmid (Schmid-Stählin *Griech. Literaturgeschichte* 1, 1, p. 291 n. 5), but we are too short of analogies to say for certain that the non-Homeric dialect requires a later date. Hesiod has Aeolisms not in Homer and Boeotian and Doric elements.[18] Will also argues (p. 126) that a Corinthian poet of the time before true Orientalizing in art could not have mastered what he calls the Ionic-Aeolian technique (i.e. of Homer). This last argument seems weak, since it is certainly fair to cite Hesiod on this occasion, and we have seen that it is difficult to date Hesiod below 700, so that he provides evidence for the early spread of the technique. Furthermore if it had reached Ischia before 700 it is hard to believe Corinth would not have heard of it.[19]

Will also interprets the Eastern references (e.g. K.17, Borysthenis) as shewing a date later than the eighth century. They belong to the period after the Milesian colonization of the seventh century, and Will cites Rhys Carpenter to support his view that the Pontus could not have been known before *ca.* 700 (p. 127 n. 3). The first part of the present paper tried to shew that Rhys Carpenter's thesis is not to be accepted.

In arguing the question of date it is surprising that Will makes no mention of Dunbabin's statement (*JHS* LXVIII, 1948, p. 67) that the best evidence for Eumelus' date is the fragment K. 13 which we have already mentioned. This is for Messenians and stresses freedom (μοῖσα . . . ἐλεύθερα σάμβαλ' ἔχοισα); Dunbabin therefore infers that it was written before Sparta's subjection of Messene,[20] though he also notes that it might have been composed during the Second Messenian War (p. 67 n. 71). None of these arguments on the date inspire great confidence, those against the eighth-century traditional date no more than those supporting it. But there is possibly sufficient

[18] See Buck, *The Greek Dialects*, Chicago 1955, p. 15.

[19] See *CR* n.s. 6, 1956, pp. 95ff.

[20] As Pausanias expressly says, IV, 4, 1 (it was under Phintas, king of Messenia a generation before the First Messenian War), and Wade-Gery already suggested (*CAH* III p. 534 n.).

uncertainty for us to understand Will's dilemma (p. 128); either the chronological tradition is false; Eumelus is a Corinthian Homeric poet but not of the eighth century, or that tradition is correct, but Eumelus is not the author of our fragments.

On the question of the Pontic references there are further considerations. The question of attribution is very important, and Pausanias at least did not consider anything genuine except the hymn for the Messenians. Will also argues that Sinope the nymph should not be derived from the Milesian colony (p. 126 n. 8) || on the grounds that minor deities were not invented as late as the seventh century and her genealogies are all from central Greece. This is not the common view (see e.g. Dunbabin *loc. cit.*) and the free invention of eponymous heroes and heroines[21] may incline us to reject Will's argument, but the truth remains that the uncertainty of Eumelus' date and of the attribution of these fragments prevent any great weight being accorded to this evidence.

Although there is no explicit reference to the Black Sea in the Iliad or Odyssey, as we have noted earlier, the attempt has been made to refer passages in the Odyssey in particular to precise localities in the Pontus. The landscape in the Nekyia for example is thought to shew knowledge of the Pontus, as are the mare-milkers in the Iliad (XIII 5-7), An account of these suggestions is given by Minns (*Scythians and Greeks* pp. 436f.), who rejects the thoroughgoing theory that the Odyssey was first localized in the Pontus and then transferred to the West. Minns himself concludes (p. 437) that some elements in the poems shew the Ionians had a good idea of the Pontus when they took shape. But none of these can be regarded as certain; they are mostly mariners' tales of no fixed locality (e.g. the description of the land of the Cimmerians of which Minns makes much) and I agree with those who think that the poet of the Odyssey had no precise geography in mind. Similarly much cannot be made of the mare-milkers in the Iliad which only shew a vague, possibly very indirect, knowledge of nomadic life.

A suggestion of Barnett[22] that there was by the end of the eighth century a trade route from Mesopotamia to Greece by land through

[21] See list in Farnell, *Greek Hero Cults* pp. 413ff.; the eponym of Lampsacus, Lampsace, would seem a permissible analogy of the right date.

[22] In his chapter "Oriental influences on Archaic Greece" in "The Aegean and the Near East," *Studies presented to Hetty Goldman*, New York 1956, pp. 227ff.

the Caucasus and then by sea from the south coast of the Pontus assumes Greek knowledge of the Pontus by this date. In fact it is the traditional foundation date of Trapezus which partly inclines Barnett to suggest this route at this time. He shews clearly the material contacts between Greece and Mesopotamia from the end of the eighth century and argues that there were two routes for these objects. The first, the caravan route across Asia Minor, is beyond dispute since Woolley's excavations at Al Mina, where the route reached the Mediterranean.[23] But Barnett considers that this route would be very dangerous when Asia Minor was continually invaded by Assyrians, Cimmerians and Treres and thus turns to his "North East passage." He finds support for his suggestion in the 'Henkelattaschen', the birds' heads attached to the rims of great bronze cauldrons, which were common in the Greek world of the orientalizing period. His argument is that such heavy unwieldy imports shew the trade was mostly seaborne, because he finds it inconceivable that they could have been carried the entire breadth of Anatolia from East to West by land.[24] These arguments for the North East passage clearly need more substantial support from excavation along the route. However, if this uncertain but possible suggestion is accepted we have evidence of Greek traders in the Pontus by *ca.* 700, for the only possible seamen in this area at this period would be Greeks.[25]

There remain the myths. A considerable number of myths were localized in the Black Sea area,[26] and might therefore be used to shew the Greeks' knowledge of the Pontus. But clearly they can only be so used if we know how early the myth grew up or how early it was placed in the Pontus, and for much of our knowledge of Greek myths we are dependent on comparatively late sources. Only Homer and Hesiod are early enough for our purposes and they locate no myth explicitly in the Pontus. On the other hand if they know a myth which was localized in the Pontus, it must shew either that the Pontus was known when they wrote or that the myth

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[23] See Barnett 234ff. and Dunbabin, *The Greeks and their Eastern Neighbours*, London 1957, chapter II.

[24] Jantzen considers they came through Al Mina, see *Griechische Greifenkessel*, Berlin 1955.

[25] Cf. Minns, *Scythians and Greeks* p. 437, "of Phoenicians (sc. in the Euxine) there is no real trace."

[26] Listed in O. Gruppe, *Griechische Mythologie und Religionsgeschichte*, München 1906, sections 136-8.

was localized in the Pontus later. Examples should bring out these principles. ||

The story of Achilles is ended in sources later than Homer by his life after death on the island of Leuke in the Euxine.[27] But since Homer makes him go to Hades (Od. XI 467ff.) we may reasonably conjecture that the Black Sea did not enter into the story known to Homer. From Aeschylus onwards Prometheus' punishment generally takes place in the Caucasus, but Hesiod does not specify the location. This too looks like an early story with a later geographical position.[28] The Amazon myth seems to have progressed similarly, for their habitat is in the Sangarios area (Iliad III 184) or Lycia (Iliad VI 181) in Homer but is then pushed steadily further East.[29] Nor is there any need to assume that the exploits of Herakles in the Black Sea were known to Homer.[30] In all these instances the myths were known to Homer or Hesiod, but the localization in the Pontus area seems to have come later.

The story of the Argonauts, however, presents more of a problem.[31] Here we have a myth well known to the poet of the Odyssey, which in the form we know it largely consists of a great voyage into the Pontus. This would imply Homer's knowledge of the Black Sea unless it can be shewn that the form of the Argo story that he knew did not involve the Euxine. The Odyssey passage is vital to the argument, so may be cited here. After describing the dangers of the

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[27] See Rose, *Handbook of Greek Mythology* pp. 242f. and n. 49, p. 252.

[28] See Rose *op. cit.* p. 72 n. 59.

[29] See Halliday, *Greek Questions of Plutarch* pp. 209ff.

[30] See Rose *op. cit.* pp. 214f. and notes.

[31] It has long been thought that the story of the Golden Fleece represents real early expeditions for metal (see e.g. Bury, *History of Greece*[3] p. 48), but though this is quite plausible, it is not a clear indication of date. A recent restatement of the view is in Barnett's chapter mentioned above. He notes (p. 221) that there are frequent references in the annals of the Urartu kingdom to the kingdom of KULHAI, and adds "this is the land of Colchis and the recent Russian excavations of burials rich in gold and silver at Trialeti in the central Caucasus, belonging to the late Bronze Age, shew that the Golden Fleece and the Argonauts need not be considered all a figment of the imagination." A remark on p. 228 shews that he considers the factual basis of the Argonaut story to be bronze age trading, but in fact the identification KULHAI = Colchians does not provide any indication of date. As he notes elsewhere, there was plenty of gold and silver in the kingdom of the KULHAI in the eighth century (see p. 229). The first mention of Colchis known to us in Greek is attributed to Eumelus of Corinth (K. 2); and thus may be of the eighth century, though this conclusion is subject to the reserves already mentioned about the date of Eumelus and attribution of his fragments.

Planktai, or wandering rocks, Circe says (XII 69ff.) "one sea-going ship alone sailed past this place, world-famous Argo, as she voyaged back from the land of Aia. And she too would have been swiftly dashed against the great rocks, but Hera sent her past because she loved Jason."

With this passage and the later tradition before him Robert tries to disengage the earliest form of the myth.[32] He discusses elements in the story which point to origin in different states and notes that Miletus is in the foreground. But if the myth is thought to originate in Miletus the outstanding role of Corinthian Medea is unexplained. Corinth must have shared in the saga's creation in a decisive way, and Corinth's colonial expansion was in the West. Two points in the story point to an earlier localization in the West. The first is in the earliest reference, the Odyssey passage; Argo comes through the moving rocks on the way back from Aia (παρ' Αἰήταο πλέουσα). Secondly the Boreades pursue the Harpies to islands off the West coast of the Peloponnese.[33] Robert explains the first as follows: if the moving rocks symbolize the entry to the Pontus, as is certainly so in later Greek writers, and Argo is thus made to come through the entry to the Pontus on her way back, the fairy-tale land of Aia is not in the East. The Argo's goal is in the West, and her return journey brings her via Ocean back through the *Symplegades*. This, he says, is the form of the myth known to the Odyssey poet, and the complete reversal of the story's geography, including the details of the episodes in the Black Sea, will be due to later Milesian mariners and poets.

It is clear that the evidence for this view is somewhat tenuous,[34] but there is sufficient to make it arguable that Homer's knowledge

[32] *Die griechische Heldensage*, Berlin 1921, III pp. 758ff.

[33] Hesiod frg. 57, from Bk. III of the *Katalogai*, see *P/W* s.v. Harpyien col. 2424.

[34] Objections or other views worth noting include the theory that Eumelus gave the Corinthian connection to the story in order to glorify Corinth in the latter eighth century, as he seems to have done with other myths (see Dunbabin, *JHS* LXVIII, 1948, pp. 66f.). Will finds the only cause of the connection with Corinth in the figure of Medea herself (see *Korinthiaka* pp. 121ff.); he suggests she may have been a widespread deity (on rather slight evidence given on p. 122) who, because she was worshipped in Corinth as well as Thessaly, where the myth originated, brought the myth to Corinth. Whether or not we accept either of these views, there is great force in Will's remark (p. 121) that the apparent Western allusions in the passage Odyssey XII, 69ff., do not alter the fact that "les Argonauts n'ont rien de corinthien."

of the Argo story does not necessarily imply knowledge of the Black Sea. And for our present purpose it might be thought sufficient to throw doubt to remove the validity of this evidence for the date of the Greek entry of the Pontus. But there is one aspect of this question which seems to deserve more detailed attention: that is the moving rocks or the rocks which clash together.

The story of the Argo and the *Symplegades* in its developed form is found in Ap. Rhod. II, 549ff., where the Argonauts send a dove through first, and thus just come through themselves with some damage to the stern. The lines 59ff. in || Odyssey XII, although the poet calls the rocks *Planktai*, and although they are not explicitly said to clash together, seem to refer to rocks like the *Symplegades*. This is generally recognized (see e.g. Robert, *op. cit.* III, 825), and will be clear if the lines are quoted. "In one direction there are overhanging rocks against which a great wave of Amphitrite roars and dashes. The blessed gods call them *Planktai*. Not even birds pass by this way, nor the shy doves, which carry ambrosia to father Zeus, but the smooth rock takes toll of them every time they pass, and the father sends another to make up the number. No ship of men which approaches this place has any escape whatever, but the waves of the sea and storms of deadly fire carry it off, together with the planks of the ships and bodies of the men." Then follows the passage about Argo already cited. It is the danger to the birds which seems to fit the *Symplegades* and no other type of dangerous rock. Robert takes Apollonius Rhodius' story of the dove sent through in advance as a later addition based on the model of the doves in the Odyssey (see pp. 829f. and 830 n. 3). He notes that the earlier version in Pindar (Pyth IV 204ff.) has no mention of doves. But this is an argument from silence and it is difficult not to be struck by the fact that the doves in the Odyssey passage seem unexplained and meaningless. Stanford (note *ad loc.*) finds them "curiously reminiscent" of the dove in Apollonius Rhodius, and since the dove there has a clear purpose, it is perhaps easier to take the Odyssey's doves as a vague reference to this story, than to explain this story as made up from the Odyssey's curiously purposeless doves.

But even if this point is not pressed, so long as it is agreed that the Odyssey passage refers to a story similar to the Argo's passage of the *Symplegades*, an interesting argument is admitted. In the areas of Greek navigation there is no place more suited to give rise to the

myth of the moving or clashing rocks than the Bosporus. Ancient geographers were right to point out in explanation of the myth that the strait seems closed from afar, and as you come nearer and move from one side to the other, it seems to open and to close.[35] When one adds to the appearance the formidable current, one sees that the Bosporus provides a very suggestive factual basis for the *Symplegades* myth. And if the myth arose from the passage of the Bosporus, then the Odyssey's reference to it, however vague, shews that Greek sailors had penetrated into the Black Sea before the Odyssey was composed.

The use of this argument chronologically depends on the date chosen for the Odyssey. This is not the place to discuss again this much discussed question. A clear and sober statement of the matter may be found in Page, *The Homeric Odyssey* pp. 146-9, where he argues that the lower limit cannot be later than 700 BC. Few I think, would now quarrel with this. It is unnecessary to emphasize the uncertainty of this argument from the story of the Argonauts. It requires the belief that the *Symplegades* myth arose from the historical experience of the passage of the Bosporus, that the Odyssey refers to this myth, and that the Odyssey was composed before *ca.* 700 BC. But as none of these beliefs is outrageously credulous or unjustified, the argument seems worth its place in the discussion.

Conclusion

This ends the consideration of the evidence for the date we seek, and there remains only the task of evaluating the conclusions this evidence suggests. The first is a negative one, we have seen that neither Rhys Carpenter's || argument about the Bosporus nor Akurgal's excavation at Sinope justify the conclusion that the Greeks did not enter the Pontus before *ca.* 650 BC, the archaeological *terminus ante quem.* Thus the tradition of early foundations of Trapezus and Sinope has not been proved wrong, though, equally, it is still without direct archaeological support.

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[35] See Eratosthenes ap. schol. Eur. Medea 2, and Dion. Byz. 3, 19ff.

My second conclusion is that these early foundation dates are on the whole supported by the other evidence and the argument from probability. The potter Istrokles, the occurrence of the Ister in the Theogony, and other less certain indications like the possible reference to the penetration of the Bosporus in the Odyssey, all point to *ca.* 700 as the latest date for this event, and the excavations at Daskyleion seem to support this, even if indirectly.

In the first place these excavations, by shewing that Cyzicus was firmly established by 700 have justified the high date for Cyzicus' first foundation in the literary tradition, and this increases the credit of the other foundation dates in the East. The excavations also provide evidence of the strength of the Greeks on the south shore of the Propontis by *ca.* 700 BC, This only supports a natural conclusion from the site of Cyzicus. The site, on the isthmus of a very considerable peninsula, would only be chosen by people able to control the whole peninsula. If the colonists of Miletus were able to settle in such strength at Cyzicus before 700, there seems no reason to believe that they were not able to enter the Pontus and settle at Sinope.

Ability to do so does not prove that they did, and we have seen above that for colonists the Pontus is a very different sea from the Propontis. But for sailors and fishermen, who knew the Propontis and the Hellespont, the passage of the Bosporus into the Pontus would be a natural extension of their activities, and those who then voyaged East with the prevailing current along the coast would see the outstanding attractions of the site of Sinope. Exploration of these coasts by fishermen is the conjecture of Cary and Warmington in their book *Ancient Explorers.*[36] Cyzicus, the Bosporus and Sinope were all famous for their fish in antiquity,[37] and in this way the Pontus and the Propontis are connected. It is therefore not unreasonable to argue from the appearance of the Greeks in the Propontis to their probable appearance also in the Pontus.

As was stated in the introduction to this paper the arguments and evidence available to us are far from satisfactory and do not allow precision or certainty. But we would not be pressing the evidence

[36] London 1929, p. 27 and notes.

[37] See *op. cit.* p. 212 n. 108; Dion. Byz. 1, 1.7, 4, 1.2 for the fish in the Bosporus and the Golden Horn; for Sinope's fish see D. Magie, *Roman Rule in Asia Minor* I p. 184 and II p. 1076.

too far if we said: the direct archaeological evidence gives us a *terminus ante quem* of *ca.* 650 for the Greek entry of the Pontus; on the other hand there is literary evidence of Greek settlements in the middle of the eighth century, and this evidence is not shaken, but is rather supported, by the indirect evidence and arguments which can be applied to the problem. ||

8. GREEK AND ROMAN SETTLEMENTS ON THE BLACK SEA COASTS: HISTORICAL BACKGROUND

The Greek settlements round the coasts of the Black Sea only came to an end in 1923, when the disastrous exchange of populations between Greece and Turkey was negotiated by the Treaty of Lausanne. These settlements had a history reaching back to the seventh century BC at the latest. Since even within the chronological limits of the Ancient World we are talking of at least a millennium of history, it is obvious that I cannot even attempt a sketch of that history here.[1] Instead, I shall discuss a small number of historical problems that seem important to me (Figs. 1 and 2).

The literary evidence for the history of the Greek settlements in the Black Sea is limited in quantity, and provides for the most part only a bare framework.[2] Archaeological investigations in the area, however, have been conducted for as long as anywhere in the Mediterranean world, with the exception of the Bay of Naples. As a result, the history of the Black Sea in antiquity is to a large extent reconstructed on the basis of archaeological data.

Naturally the archaeological data are used more easily and straightforwardly for relatively simple and basic historical purposes. Here chronology stands out. Where there has been sufficient exploration to allow confidence that the earliest material has been discovered, archaeological material is very valuable in establishing the date of a settlement's foundation. For many Greek colonies in the Black Sea, we also have foundation dates transmitted by literary sources. These literary foundation dates have generally been well confirmed by archaeology for sites on the West and North coasts. Here Greek colonization clearly began about the middle of the seventh century BC.

On the South coast, however, we have a higher literary foundation date, 756 BC, for Trapezus, and this implies a similarly high

[1] This paper is published with very few changes from that delivered at the Annual Meeting at the Archaeological Institute of America in Washington on December 30th 1993.

[2] More detail on the history and sources of Greek Colonisation may be found in Graham 1982. For the Pontus, see 122-30.

date for Sinope, since Trapezus is reported to be a colony of Sinope. This eighth century foundation dates have not been confirmed archaeologically. To my knowledge there have been no scientific excavations at Trapezus. At Sinope, small digs have been carried out, both in the city and the cemetery outside. The earliest Greek pottery discovered is a sherd of Middle Wild Goat ware, which may be dated to the last third of the seventh century (Graham 1982:123). We can have no confidence that the earliest material from the Greek colony has necessarily been found, however, because the modern town lies precisely on top of the ancient city and because of the position, character and subsequent use of the cemetery (see Graham 1958:32-33).

The picture that we now possess from the archaeological evidence has led some scholars to reject the eighth century dates stated and implied by the literary evidence for Trapezus and Sinope. Boardman, for example, thinks that Greek colonisation in the Pontus did not begin till the middle of the seventh century (Boardman 1980:240-242; Boardman 1991:387-390). I have stated my opinion that it is bad in principle to reject statements in our literary sources on the basis of an archaeological || *argumentum e silentio* (Graham 1982:123) and I tried to advance the discussion by pointing to a few pieces of Greek pottery found in the Pontus which seemed earlier than the second half of the seventh century, but which were either ignored or explained away (Graham 1990:53-54). Boardman (1991) has taken up the challenge and reconsidered the pottery in question. The small geometric hydria, called a hydriske by Boardman, said to come from Berezan, classified as Middle Geometric II by Coldstream, and thus dating to *c.* 820-760, Boardman continues to exclude from the argument, on the ground that it has only a 'dealers' provenance'. The rim of a Late Geometric kotyle, identified by Coldstream as a Euboean copy of a Corinthian type, and dated *c.* 750-720, belongs to a group of sherds given by the excavators of Histria to C. A. Rodewald, and by him to the Museum of Classical Archaeology at Cambridge. Boardman has recognised another similar sherd in the same group, and has very acutely suggested that there has been contamination in the Cambridge museum between Rodewald's Histrian sherds and a group of sherds from Al Mina, which came into the museum at about the same time. Both groups of sherds were freely used for teaching purposes. The Euboean Late Geometric sherds would be entirely at home in the group from Al Mina. I regard this as a bril-

liant solution, and am happy now to agree that the Late Geometric kotyle sherd did not come from Histria. ||

The Cypriot White Painted sherds found at Histria, which used to be classified as Cypro-Archaic I, thus dating to before *c.* 660, Boardman now reclassifies as Cypro-Archaic II, which brings the date down well into the sixth century. As for the amphora found at Histria, which Alexandrescu (1978:63 n. 256 Pl. 26) thought similar to G2-3 ware, which would imply a date no later than the first half of the seventh century, Boardman does not accept Alexandrescu's comparison. In spite of its "broadly sub-geometric decor," according to Boardman (1990:389), "it's added red lines and heavy lip" bring it "closer to the sixth century."

It is certainly valuable to have such a great expert as Boardman re-examine this pottery, but these arguments are not the most important part of the chronological controversy. Until we have archaeological evidence from the Greek sites in the South coast of the Pontus equivalent in quantity and quality to what we have from the West and North coasts, it still seems to me bad method to reject the eighth century literary foundation dates. New discoveries could so easily prove that rejection to be wrong.

To turn to more specific chronological questions, it is firmly believed by the local scholars that Olbia was first settled by Greeks considerably later than Berezan. In the *Cambridge Ancient History* I pointed out that we have one important piece of pottery from Olbia as early as anything from Berezan, and that the much greater quantity of seventh century pottery from Berezan could be explained by the facts that the early levels at Olbia have been relatively little investigated, and much of the ancient Lower Town is now below the Bug river (Graham 1982:125-126).[3] My conclusion that Olbia was settled as early as Berezen has been, as far as I know, universally rejected by Russian scholars (see Sogomonov 1984:165; Vinogradov 1989:43 n. 57). I am interested to see, however, that, whereas the beginning of the settlement at Olbia was generally dated in Russian scholarship to the second quarter of the sixth century, when I was working on my *CAH* Chapter, now Vinogradov (1989:36) is dating it to *c.* 600, which is already coming closer to the likely date of Greek settlement on Berezan. I still think that more archaeological

[3] For recent excavations at Olbia, including the Lower Town, see Hind 1992-3, 94-5.

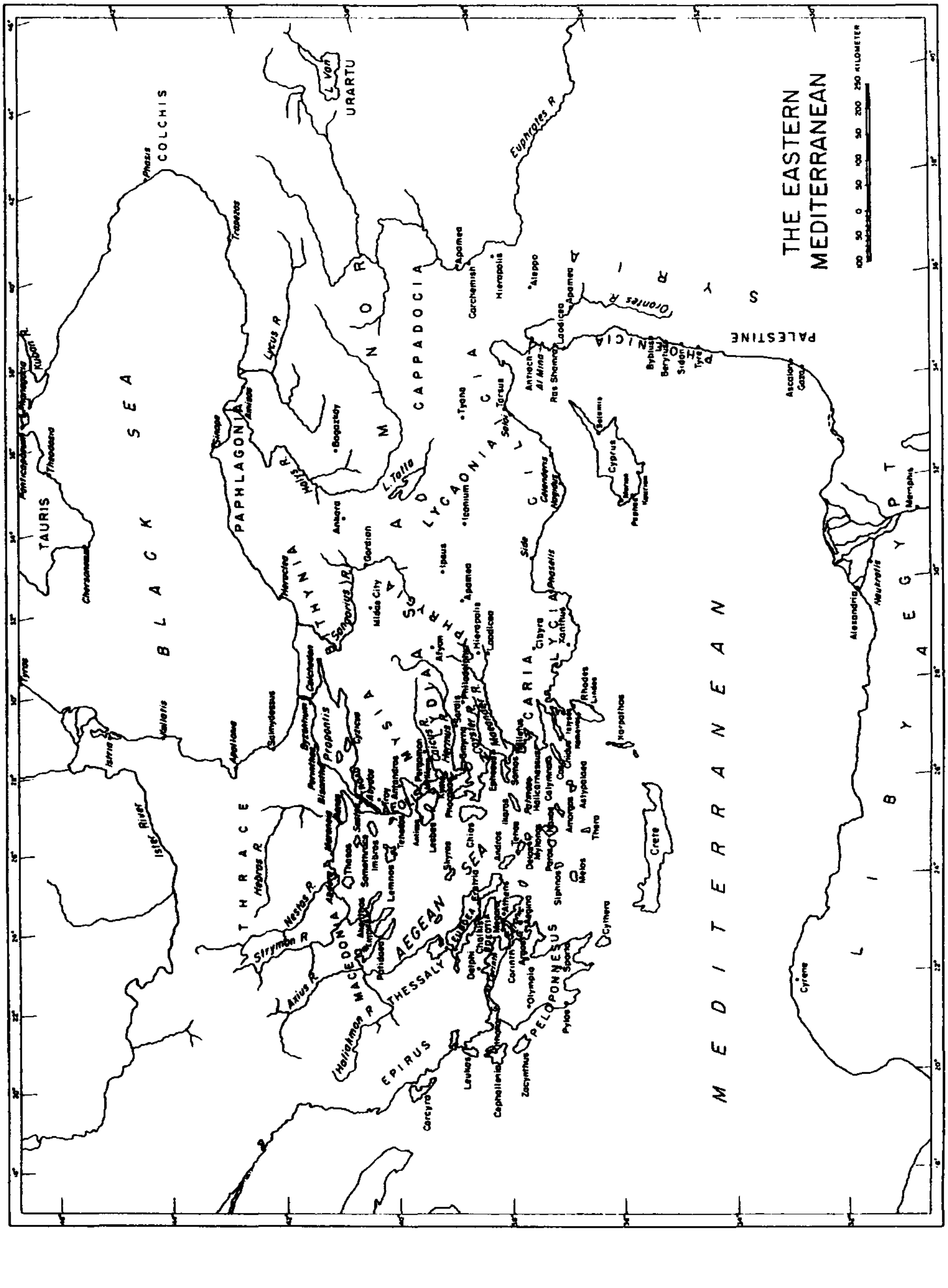

Figure 3. Map of the Eastern Mediterranean

investigation at Olbia may one day show that the Eusebian foundation date applies there too.

Callatis is stated by Ps. Scymnus to have been founded in the first regnal year of a Macedonian king called Amyntas. This gives us two possibilities, either about the middle of the sixth century BC, if it was Amyntas I, or 394/3, if it was Amyntas III.[4] Presumably Demetrius of Callatis, one of the chief sources for Ps. Scymnus on the Pontus, mad the matter clear, but the vital information was omitted in the abbreviated account we possess. There has now been sufficient archaeological exploration of the site, however, to prove quite certainly that the later date is correct (Ulanici 1974:191-195; Hind 1983-4:75). Yet this solid discovery is still not known by some historians of Greek colonisation.

When we turn from relatively simple historical matters like chronology to the more complicated and more significant issue of the interrelation of the Greek colonists with the native population, we find a host of important topics and problems. There are the political relations of domination, protection and subjection between Greek and native states; the examples of mixed communities; influences like language, script, coinage, the arts and crafts; and, perhaps most important of all, mutual influences in religion. In all these cases archaeological data are eagerly used by historians. Yet, when we leave the articulate literary and epigraphical sources, the message of the archaeological data is often obscure. Even the most basic question, the identification of the people we are dealing with, is often difficult to answer with certainty, both in settlements and in graves. ||

On the basis of analogies, I think it probable that the contracted (crouched) burials with no grave goods in the cemeteries of the Greek cities will be those of native slaves. It would be nice, however, if we could find a skeleton with leg irons still in place, such as have been found in some Greek cemeteries,[5] which would prove the matter. In these connections the grave of Tychon the Taurian at Panticapaeum has naturally always attracted attention. The grave belongs to a

[4] For the dates of Amyntas III, see Hammond in Hammond and Griffith, 1979, 168. (Amyntas II, 'The Little', seems to have reigned briefly within the same year, 394/3, so, even if it were he, the chronological conclusion is unchanged.) Amyntas I, who died about 498, ruled for 49 years, according to the Macedonian king list preserved in Diod. 7.5.1; see Borza (1990:98, 103).

[5] As, for example, at Acanthus; see French 1990-91, 49.

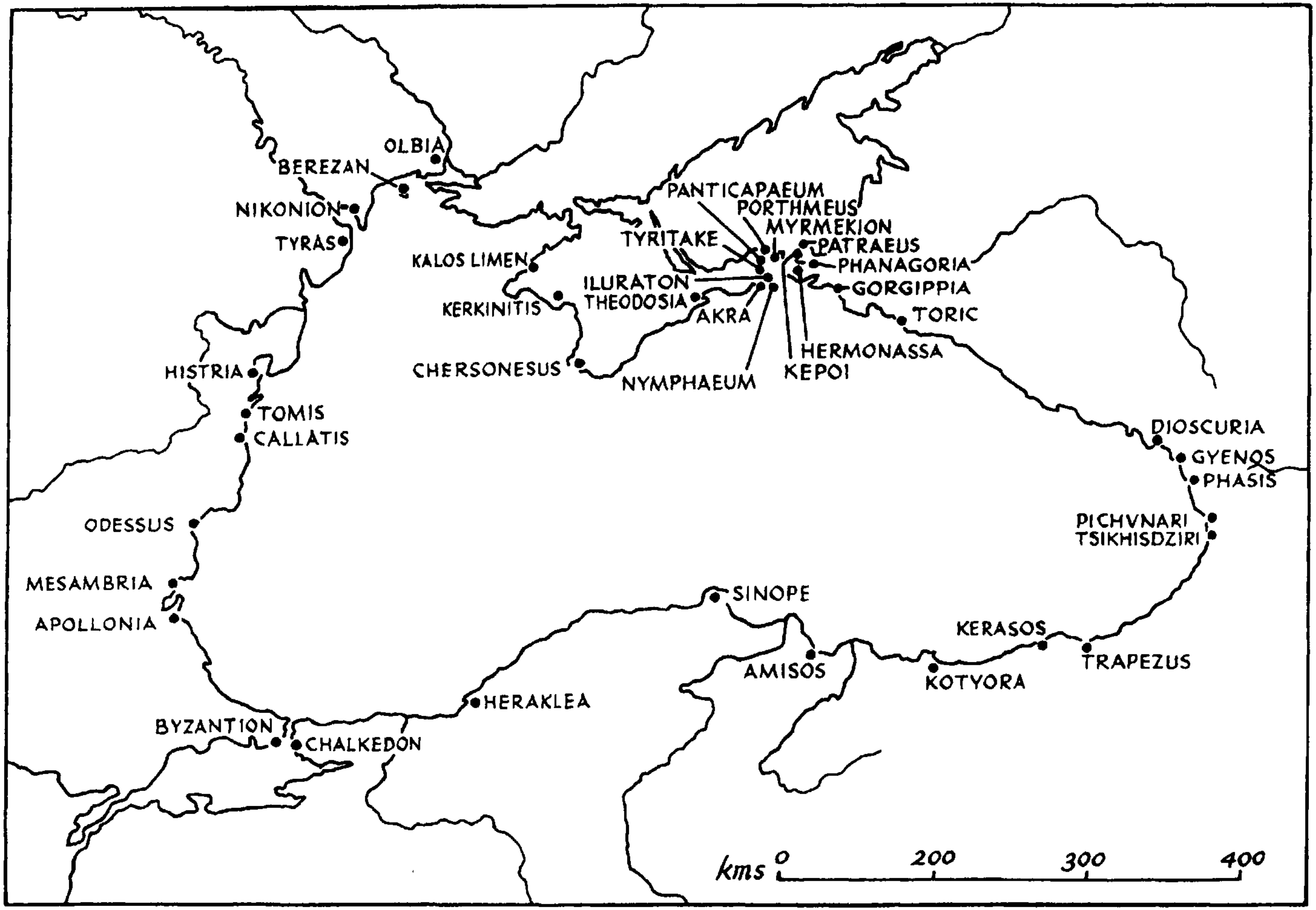

Figure 4. Map of the Black Sea (after Tsetskhladze and De Angelis, forthcoming)

cemetery of the sixth and fifth BC, in which most of the burials appear to be straightforwardly Greek.[6] Tychon's body was covered with red ochre before burial and no grave goods were found (Shkorpil 1904:80, grave no. 35).

The grave was marked by an inscribed stele containing an elegiac grave epigram,[7] and Tychon, though he bears a common Greek name, is explicitly described as a Taurian. Previous discussions known to me have rightly dismissed the idea that Tychon could be a slave, despite the Greek name and native burial rite. The grave epigram, an unusual honour in cemeteries at Panticapaeum at this date, excludes the interpretation. The conclusion that he was a free man of some distinction has been used to characterise the rights of non-Greeks in this period at Panticapaeum (Blavatsky 1964:50-51).

There is one category, however, which I have not found considered, that of visitor. The language of the epigram is comparable to one by Simonides for Brotachos of Gortyn,[8] who explicitly died abroad on a trading journey. Note Κρῆς γενεὰν and Ταῦρος ἐὼν γενεήν. If Tychon were a distinguished visitor at Panticapeum, all aspects of his burial can be understood. Furthermore, his Greek name would suit well a *xenia* relationship with a Greek family (see Herman 1990).

These interesting problems in the relations between Greek colonists and native peoples are characteristic of Greek colonial areas, and the combination of literary, epigraphic and archaeological sources make the Black Sea a rich area for such investigations. There is another area where evidence from the Black Sea seems to me to make an important contribution to the economic history of the Greek world in general.

The arrow and fish/dolphin coins found in vast numbers, not only in the Greek colonies, such as Apollonia Pontica, Histria, Olbia and

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[6] For description of the excavations of the cemetery, see Shkorpil 1904 and 1909.

[7] *CEG* 175:

Τύχ|ων|ος.
σῆμα|τι τῶιδ'| ὑπόκει|ται ἀνὴρ| [π]ολλο[ῖ]σι ποθε|νὸς|
Ταῦρος ἐὼν γενεήν. τὄνο[μ]|α δ' ἐστὶ Τύχων.

[8] Anth. Pal. vii 254b:

Κρῆς γενεὰν Βρόταχος Γορτύνιος ἐνθάδε κεῖμαι.
οὐ κατὰ τοῦτ' ἐλθών, ἀλλὰ κατ' ἐμπορίαν.

For a list of the epigrams in *CEG* which record the nationality of someone who died in a foreign land, see Day (1989:18 n. 17).

Berezan, but also in the native hinterland, are proved to be money by their use as 'Charon's obol' in graves (see Zograph 1977:188-193; Lapin 1966:144-145).[9] Graves and other archaeological contexts show that they were current already in the sixth century BC. Their lack of intrinsic value and vast numbers show that these coins were made for the small exchanges of everyday life. In Colchis the so-called 'Kolkhidki', which are very small, nominally silver coins, are also found in enormous numbers. Their weight and silver content vary wildly. We have dating evidence to show that these coins, too, were already current in the sixth century.[10] In both these coinages we seem to see money with no intrinsic value, 'fiduciary' or 'fiat' currency, like the paper money of modern times.

The widespread use of such money seems to me to show quite incontrovertibly that the economy round the shores of the Black Sea was fully monetized by the sixth century. Money was the universal means of exchange, used in all transactions, however small in value. I would also conclude, on the basis of an obviously very strong *a fortiori* argument, that the same conditions must have obtained in the Greek world generally. So much for the widespread current notion that Greek society was not fully monetized in Archaic and Classical times. ||

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[9] For more recent finds, see Hind (1992-93:85, 89).

[10] For the Kolkhidki I rely on the account by Hind 1971, "East Coast" 22-8. There is a brief description of the Greek coins from Colchis and Doundoua 1982. For the Kolkhidki, see 55-6, where the discovery of interesting new types, of exceptionally small size, in a grave at Pichvnari is mentioned.

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9. MEGARA HYBLAEA AND THE SICELS

Of the many deficiencies in our evidence for Greek colonization in the Archaic period, one of the worst is the lack of good information about the native peoples among whom the Greeks established their new settlements. We are fortunate if we know more than the general name of the people involved. The size and nature of their political units and institutions must usually be guessed on the basis of probabilities and analogies. So the initial relations between colonists and natives can only be grasped, at best, in a vague and general manner.

Yet we do know that these relations ran the gamut from most hostile, when colonization was achieved by the forcible expulsion of the native inhabitants, to most friendly, when the colony was established as the invitation of the local people. The best example of the latter phenomenon, had colonization resulted, would be provided by Herodotus' story (1.163) of Arganthonius, king of Tartessus, and the Phocaeans. In this instance the Phocaeans traded regularly with Tartessus and became so beloved by the local king, that he invited them to settle in his country. Although the invitation was refused, our evidence does provide a small number of other cases where Greek colonies were established at the invitation of the native people. The earliest and perhaps the best attested is that of Megara Hyblaea. ||

Megara Hyblaea was founded on the East coast of Sicily by settlers from Megara in Greece in *c.* 728 BC.[1] This date can be reached by very close calculations on the basis of Thucydides' statements (6.4.1-2).[2] The higher date, *c.* 750, which was proposed by Vallet and Villard and widely accepted until recently, was chiefly based on inferences from archaeological material, which have since been found to be incorrect, and has been publicly retracted by its proposers.[3]

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[1] See CAH III^{2}.3, 106-8; J. Bèrard, La colonisation grecque de l'Italie et de la Sicile dans l'antiquité, second edition (Paris 1957) 109-16.

[2] See T. J. Dunbabin, The Western Greeks (Oxford 1948) 435-8.

[3] G. Vallet and F. Villard, "Les dates de fondation de Mégara Hyblaea et de Syracuse," BCH 76 (1952) 289-346, and, for the retraction, Insediamenti coloniali

Local Ethnopolitical Entities of the Black Sea Littoral in the Seventh to Fourth Centuries BC (1988), 304-321

Our most authoritative source, Thucydides, states that the colonists were conducted to their final site by the Sicel king, Hyblon, who gave them the land (6.4.1). This statement is in general terms strongly confirmed by topography and archaeology. The low-lying site has no natural defences on the westward, landward, side, and the low escarpments on its eastern, northern and southern sides where the land rises from the sea and two small rivers, which have been considered natural defences by some authors,[4] are in truth barely worthy of such a description. It is more realistic to regard the whole side as undefended by nature and its choice undoubtedly reveals the colonists' confidence on the goodwill of the native inhabitants. Archaeological investigations have also shown that human settlement of the site began with the establishment of the Greek colony, which was, therefore, planted on virgin, unoccupied, land.[5] These facts would lead us to postulate circumstances of settlement similar to those described by Thucydides, even we did not possess his explicit statements.

If the general circumstances of Megara Hyblaea's settlement are pleasingly clear, it is much more difficult to achieve the sharper definition and greater detail that we are bound to seek. For this a close examination of the literary sources is the essential beginning. We have four main passages to consider: Thuc. 6.4.1; Polyaenus, Strat. 5.5; Strabo 6.267; and Ps.-Scymnus 270-79. The two last were derived, whether or not directly, from Ephorus, and are printed as his fragment 137 in Jacoby's collection (FGH 70 F 137).[6] Ephorus' account of the early colonization of Sicily diverges significantly from that of Thucydides (as also from Polyaenus), so we must subject it to close scrutiny.

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greci in Sicilia nell' VIII e VII secolo A.C., Cronache di Archeologia e di Storia dell' Arte 17 (1978) 150-2.

[4] E.g. Bérard, *op. cit.* 114 and Villard, 'Mégara Hyblaea', Mél.Arch.Hist.École Franc Rome 63 (1951) 7-52.

[5] Compare Villard's description of the stratigraphy, *op. cit.* 10-12.

[6] The unmistakable similarities in the passage Ps.-Scymnus, 264ff. show that it too came from Ephorus, even though his name is not mentioned, whether directly or through an intermediary. (Both Jacoby, FGH Introduction to the commentary on 70, Ephorus von Kyme, pp. 34f., and G. L. Barber, The Historian Ephorus, Cambridge 1935, 175-6, believe that the author of the geographical poem did not use Ephorus directly.) The main importance of the Ps.-Scymnus passage is that it establishes the dating given by Ephorus, since there is a corruption at this point in the MSS. of Strabo; see Jacoby's commentary on the fragment and Bérard, *op. cit.* 76 n. 1. We have two passages which attest Ephorus' date for the Return of the

Here is the translation of the relevant passage:—

. . . . Naxos and Megara . . . Ephorus says that these were the first Greek cities to be founded in Sicily, in the tenth generation after the Trojan War. For, before that, || the Greeks were afraid of the bands of Etruscan pirates and the savagery of the native people there, so that they did not even sail there for trade. But Theocles, an Athenian, being carried off his course and brought by winds to Sicily, perceived the weakness of the people and the quality of the land. When he returned, he could not persuade the Athenians, but he collected many of the Chalcidians in Euboea and some Ionians, and also some Dorians, of whom the majority were Megarians, and thus set sail. The Chalcidians founded Naxos, the Dorians Megara, which was formerly called Hybla.

If we compare this account with that of Thucydides (6.3-4), we find four important differences. Firstly, chronology. We do not know with complete certainty Ephorus' date for the Trojan War, but it was probably *c.* 1150 BC.[7] Nor do we know with complete certainty how many years he allowed to a generation, but it was probably thirty.[8] In that case, this date for the beginning of Greek colonization in Sicily was about 850 BC, some 120 years earlier than that of Thucydides.[9] Secondly, priority. Ephorus states that Naxos and Megara were the first Greek colonies to be founded, while Thucydides makes Naxos the first and Syracuse the second. Thirdly, the founders, personnel and circumstances of the colonization of Naxos and Megara. Ephorus represents the two colonies as the product of a single mixed

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Heraclidae, Diod. 66.76.5-FGH 70 T 10, and Clem.Alex. Strom. 1.139.3-FGH 70 F 223. From Diodorus we obtain the date *c.* 1090, from Clement *c.* 1070. Although Jacoby regards this as a serious difference (commentary to FGH 70 F 223), there is something to be said for Molly Miller's view that there is not necessarily a conflict here, since the two calculations might have been made from two different events within the obviously extensive Ephoran history of the Return (The Sicilian Colony Dates, New York 1970, 86). In any case, Jacoby's own forthright solution of the problem by emending Diodorus' text is hardly appealing.

[7] Ephorus apparently dated the Trojan War sixty years before the Return of the Heracliadae; see Strabo 13.582 and Jacoby, *ibid.*

[8] Barber was able to show from Ephorus' Spartan chronology that he probably reckoned thirty years to a generation, *op. cit.* 171-2.

[9] Jacoby's 'letzte Drittel s. VII' (commentary on FGH 70 F 137) is presumably some kind of slip, since he thinks that Ephorus' chronology is 60-80 years higher than that of Thucydides. There have, of course, been many differing calculations of Ephorus' colonial chronology; see e.g., Ziegler, RE s.v. Σικελία, 2494f.; but it is common ground that it cannot be reconciled with that of Thucydides.

enterprise led by Theocles, and including Chalcidians, Ionians and Dorians (of whom the majority were Megarians); according to Thucydides, the colonization of Megara Hyblaea by Megarians under the leadership of Lamis was a quite separate venture from the settlement of Naxos by Chalcidians under Theocles. Fourthly, Theocles' origin. Ephorus' statement that Theocles was Athenian was opposed, not only by the unmistakable implication of Thucydides (6.3.1), but also by the plain statement of Hellanicus (FGH 4 F 82), that he came from Chalcis.

These differences are all substantial, and in all of them, Thucydides' account is either demonstrably or most probably superior. In a careful criticism of the Ephoran passage, Bérard noted that its divergencies from Thucydides can mostly be explained as resulting from a pro-Athenian outlook, and from the confrontation between Ionian and Dorian in Sicily during the fifth century.[10] Ephorus' pro-Athenian bias has long been recognized.[11] Such attitudes would account for the Athenian origin attributed to Theocles, the 'Ionians' who supposedly took part in the first colonial || expedition, and the exclusion of Syracuse. These tendencies add to the doubts inspired by Ephorus' erroneous chronology, and make suspect the historical value of the whole passage.

In addition to these general considerations, there are details in the account of Theocles' discovery of Sicily that look false. Fear of Etruscan piracy has often been thought anachronistic at such a high date,[12] though that is, perhaps, not capable of proof. The story that Theocles was blown off course was seen as a topos by Bérard,[13] who compared Colaeus' discovery of Tartessus, as described by Herodotus (4.152). He could have added the similar experience of Corobius (Hdt. 4.151).

Bérard also argues that, if Cyme was already founded before the Greek colonization of Sicily, the anecdote must be fabricated. It is certainly true that the reasons Ephorus gives—Etruscan piracy and

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[10] *Op. cit.* 75-9. Bérard's low estimation of the Ephoran passage has, unfortunately, not deterred other scholars from taking it seriously; see, e.g. Vallet and Villard, *op. cit.* 309-18; A. Schenk, Graf von Stauffenberg, Trinakria (Munich and Vienna 1963) 127-9; L. Bernabò Brea, 'Il crepuscolo del re Hyblone', Parola del Passato 23 (1968) 161-86.

[11] Barber, *op. cit.* 66, 84-105, 185.

[12] See Bérard, *ibid.*

[13] *Ibid.*

fear of the savagery of the inhabitants—would seem to imply that, not only Sicily, but also the area beyond the Straits of Messina, at the least, should have been terra incognita to the Greeks at this time. To show that Ephorus' account is false, therefore, it only requires to be demonstrated that the foundation of Cyme and/or Pithecusa antedated that of Naxos.

If we ignore the obviously erroneous date in Eusebius, 1050 BC, we are left with only relative indications of the foundation dates of both Cyme and Pithecusa in our literary sources. Strabo says that Cyme was the oldest of the Greek cities in Italy and Sicily; Livy that the Chalcidians first settled Pithecusa, and then transferred to Cyme; while Thucydides states that Zancle was founded first from Cyme.[14] Archaeological evidence found to date does not support Strabo. The earliest material found at hellenic Cyme is of Late Geometric (LG) II (Early Protocorinthian) date, i.e. *c.* 725-700 BC, whereas LG I material (*c.* 750-725) has been found at several Sicilian colonies, in addition to Pithecusa.[15] So, if we were to accept this archaeological evidence as decisive, we should conclude that Cyme was certainly not the oldest Greek city in the West. However, there remains the indirect argument from Zancle. At Zancle material of LG I date has been discovered. If we combine this with Thucydides' statement, we obtain a foundation date for Cyme of *c.* 750-725 or earlier. Nevertheless, these considerations show that it is by no means certain that Cyme was founded before the first || Greek colonies in Sicily.

If the foundation date of Cyme is uncertain, so also is that of Pithecusa. The only literary indication is Livy's statement given above, according to which Pithecusa was founded before Cyme. Archaeological investigations have yielded LG I material in abundance, which gives a firm terminus post quem non of *c.* 750-725 BC. A small number of sherds found in an unstratified context have been identified as

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[14] Strabo 5.243; Livy 8.22.5-6; Thuc. 6.4.5.

[15] For Cyme, see J. N. Coldstream, Greek Geometric Pottery (London 1968) 326; W. Johannowsky, "Problemi relativi a Cuma arcaica," Contribution à l'étude de la société et de la colonisation eubéennes (Cahiers du Centre Jean Bérard II, Naples 1975) 98-105, at 102. For LG I material at Pithecusa, see below; at Naxos, Leontini and Megara Hyblaea, see CAH III^2.3, 104, 108; at Syracuse, see P. Pelagatti, "Siracusa. Elementi dell' abitato di Ortigia nell' VIII e nel VII secolo a.C.," Insediamenti coloniali greci etc. (n. 3 above) 119-133, at 132-3; at Zancle, see G. Bacci, "Ceramica dell' VIII e VII secolo a.C. a Messina," Insediamenti coloniali greci etc. 100-103.

Middle Geometric (MG) II, i.e. *c.* 800-750, by one authority, but the classification and date are disputed.[16] Buchner has, in any case, long argued that the foundation date should be earlier than LG I, because of the great size of the city in that period. He contends that evidence of this supposed earlier occupation has not been found, because only a small proportion of the necropolis has been explored, and there has been no excavation on the top of Monte Vico, which was obviously the first area to be settled.[17] This is a strong argument, but not by itself decisive. So it remains possible that Pithecusa was founded in the same period, LG I, as the first colonies in Sicily.

Bérard's argument against the veracity of Ephorus information is, therefore, not quite perfect. However, we can use Cyme in another way to condemn Ephorus. It is all but certain that Strabo's statement (5.243), that Cyme was the oldest Greek city in Italy or Sicily, is itself derived from Ephorus, directly or indirectly.[18] In that case, whatever the true chronology may be, Ephorus' account of the discovery of Sicily by Theocles is convicted of gross implausibility. Ephorus could apparently believe that Greeks knew nothing of Sicily, although he knew that they had often passed it on their way through the Straits of Messina; just as he apparently believed that Etruscan pirates frightened them away from Sicily, but not from the Tyrrhenian Sea.

This examination has shown that Ephorus' account is entirely worthless. We may, therefore, combine our attention to Thucydides and Polyaenus, who can be seen to represent, in spite of some differ-

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[16] See D. Ridgway, "The foundation of Pithekoussai," Nouvelle contribution à l'étude de la société et de la colonisation eubéennes, Cahiers du Centre Jean Bérard VI (Naples 1981) 45-60. For a different opinion about these sherds, placing them rather in the LG period, see Neeft's remarks, cited by Ridgway, 49n. 21 and 50. For the view that some chevron skyphoi are of LG date, see C. W. Neeft, "Corinthian fragments from Argos at Utrecht and the Corinthian Late Geometric Kotyle," Bulletin Antieke Beschaving 50 (1975) 97-134, at 121; Descoeudres in J. P. Descoeudres and R. Kearsley, "Greek pottery at Veii: Another Look," BSA 78 (1983) 9-53, at 11-29.

[17] G. Buchner, "Nuovi aspetti e problemi posti degli scavi di Pithecusa con particolari considerazioni sulle orificiere di stile orientalizzante antico," Contribution à l'étude etc. (n. 15 above) 59-86, at 66-7.

[18] See Jacoby, FGH commentary on 70 F 134; K. J. Beloch, "Le fonti di Strabone nella descrizione della Campania," Atti della R. Accademia dei Lincei, Memorie della classe di scienzi morali, storiche e filologiche, 3 ser. 10 (1881-2) 429-48, at 436-7. The main arguments for the attribution to Ephorus are: 1) the unusual participation of Aeolian Cyme in the foundation, stated in the next sentence, points

ences, the same basic tradition. Unfortunately, however, both contain difficulties of detailed interpretation.

Thucydides tells us that, when the Megarian colonists, under the leadership of Lamis, arrived in Sicily, they settled first at a place called Trotilum, above the river Pantacyas. A probable identification for ancient Trotilum is a site above the small bay of La Bruca, on the South side of the Gulf of Catania.[19] || The site offered the colonists a refuge and small harbour, but no great possibilities, and Thucydides relates that they left it and formed a joint community for a short while with the Chalcidian colonists at Leontini. When they were expelled from there by the Chalcidians, they settled at Thapsus, a small peninsula (the modern Magnisi) in the Gulf of Augusta, which is attached to the main island by a very narrow isthmus.

Polyaenus' information from his collection of 'stratagems' concerns the part of Thucydides' account just paraphrased. He tells of two clever tricks perpetrated by Theocles, oikistes of Leontini, both of which involved the Megarian colonists. Theocles and the Chalcidians had formed a joint settlement at Leontini with its previous Sicel inhabitants. In his first trick, Theocles admits the Megarians on condition that they expel the Sicels, which the Chalcidians could not do, because they were constrained by their oaths. When the Megarians had duly expelled the Sicels, they in turn formed a joint settlement with the Chalcidians, but not for long. After six months the Chalcidians performed a great sacrifice to the gods, which Theocles claimed they had vowed beforehand, and asked the Megarians to lend them their arms for the procession. When the unsuspecting Megarians agreed to this, the Chalcidians occupied the agora and the Megarians were forced to leave the city without arms. They took refuge in Trotilum for one winter, the length of time allowed them by Theocles and the Chalcidians.

Since it is clear that Thucydides' account is pared down to essentials, there is no real difficulty in accommodating to it Polyaenus' details of what happened to the Megarians at Leontini—even if they may seem somewhat anecdotal. The two authorities are only in actual

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unmistakably to the man who notoriously tried to increase the fame of his native city; and 2) the fact that Ps.-Scymnus (238-9) gives the same information. For Ephorus is a major authority for the western part of that works; see Jacoby's introduction to his commentary on Ephorus (FGH 70) p. 35.

[19] Bérard, *op. cit.* 112; Dunbabin, *op. cit.* .18.

conflict about the place to which the expelled Megarians retired, Trotilum according to Polyaenus, but Thapsus in Thucydides. We can be sure that they did spend time on Thapsus. Not only is the peninsula an ideal refuge, but we have archaeological confirmation of their period of residence.

Among the many burials of Bronze Age date on the peninsula, Orsi discovered a later grave at a higher level. It contained two skeletons, two Corinthian cups of Late Geometric date and a pair of bronze tweezers.[20] This grave has often been called the grave of Lamis, since Thucydides tells us that the Megarians' || oikistes died while the colonists were on Thapsus,[21] but this identification is questionable. If, as seems most likely, the double deposition represents a burial of man and wife, it may seem improbable that it was the grave of an oikistes. Is it likely that the colonists would have left their founder in this very simple grave? Would they not rather have re-buried him in a prominent position in the city which became their permanent home.[22] So this grave should probably not be called the grave of Lamis. Nevertheless, it does unquestionably bear witness to the brief Megarian occupation of Thapsus.

We must conclude, therefore, that Trotilum's appearance in Polyaenus is due, either to a simple confusion with the colonists' initial place of settlement, or to compression on Thucydides' part, which led to his omission of a short-lived stage in the colonists' story. In either case the difference is hardly fundamental. It is much more noteworthy that Thucydides and Polyaenus agree on the important fact that the Megarians shared for a time in the Chalcidian colony of Leontini. So it seems to be legitimate to use Polyaenus in any attempt to reconstruct the history of the Megarians' colonial venture, even if few would be ready to press all his details.

It is very unfortunate, therefore, that the beginning of his account is mutilated by textual corruption.[23] The intrusion of 'the Plataeans' (ἐπεὶ δὲ καὶ Πλαταιεῖς ἐκ Μεγάρων), found in both manuscript F, which is thought to be the archetype of our tradition, and the vulgate, is

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[20] P. Orsi, 'Thapsos', Mon.Ant. 6 (1895) 103-4, Plate IV no. 16; Vallet and Villard, *op. cit.* 337.

[21] Thuc. 6.4.1; cf., e.g., Dunbabin, *op. cit.* 19.

[22] See my remarks in "Religion, women and Greek Colonization," Atti, Centro ricerche e documentazione sull' antichità classica 11 (1980-1, published Rome, 1984) 299f.

[23] See the text and apparatus of the Teubner edition.

beyond rational explanation, and Woelfflin's bold solution of replacing their name by that of the oikistes attested by Thucydides (ἐπεὶ δὲ καὶ Λάμις ἐκ Μεγάρων), seems justifiable. The corruption in Πλαταιεῖς presumably accounts for the form of the verb προσβάλλουσιν in the vulgate, instead of the προσβάλλειν of F. But that infinitive is by itself ungrammatical (and Polyaenus writes a simple, correct, Atticizing Greek), so Woelfflin made the conjectural addition of <παρεσκευά ζετο>, to create the following complete clause: ἐπεὶ δὲ καὶ Λάμις ἐκ Μεγάρων ἀποικίαν ἀναγὼν τῇ Λεοντίνων προσβάλλειν παρεσκευάζετο. If we accept this reading, we must translate: 'And when Lamis also brought a colony form Megara, and was preparing to attack the city (or land) of Leontini'. Here προσβάλλω with the dative has its most common significance, 'to attack'. However, the rest of the sentence hardly suggests a situation of potential || military conflict between Megarians and Chalcidians at this stage, so it may be better to understand προσβάλλειν here as 'to land', which is also a normal, if less frequent, meaning of προσβάλλω with the dative.[24] In that case, we could supply a word meaning 'wished' or 'tried' instead of παρεσκευάζετο, and translate: 'And when Lamis also brought a colony from Megara, and tried to land on the territory of Leontini'.

So much for the textual problems in Polyaenus. The uncertainties of interpretation in Thucydides all occur in his account of the final vicissitudies experienced by the Megarians. After they settled on Thapsus, their oikistes Lamis died, but the others, to use Thucydides' words, ἐκ τῆς Θάψου ἀναστάντες Ὕβλωνος βασιλέως Σικελοῦ προδόντος τὴν χώραν καὶ καθηγησαμένου Μεγαρέας ᾤκισαν τοὺς Ὑβλαίους κληθέντας. There are two words in the passage, the significance of which requires discussion, ἀναστάντες and προδόντος.

The verb ἀνίστημι, when applied to peoples, cities or nations, has a common significance, 'to expel', 'to force to migrate', active in transitive tenses and passive in intransitive.[25] Thucydides uses it frequently in this sense, and his rare uses of the verb in the middle sense, meaning 'to rise' (for example, in order to speak) have quite different contexts.[26] We should, therefore, agree with those who translate ἀναστάντες in this passage as 'having been expelled', and must

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[24] Compare Thucydides' use of the word in this sense, in the context of colonization, at 6.4.6; προσέβαλον Σικελίᾳ.

[25] Liddell and Scott, s.v., A.III.2 and B.II.2.

[26] E.g. 6.4.1; 8.76.3.

conclude that Thucydides knew that the Megarians had been driven out of Thapsus, even if he does not tell us by whom.[27]

The word προδίδωμι has two basic meanings in classical Greek. One, much the commoner, is 'to betray, forsake, desert etc.'; the other is 'to pay first, pay in advance'.[28] This second meaning is employed, for example, by Xenophon at Hell. 1.5.7 and by Polybius at 31.27.5.[29] Nevertheless, this passage of Polybius and one other, 36.3.1, have been used to justify the translation of Thucydides' προδόντος as 'handed over'.[30] In fact, neither passage supports such a translation. The first should be quite differently explained, as above. In the second, the normal meaning 'betray' makes perfectly good sense, Schweighäuser saw in his note on the passage. The Greek runs, according to the text of the MSS., τῶν Ἰτυκαίων ... τοῖς Ῥωμαίοις τὴν ἑαυτῶν προδιδόντων πόλιν. Clearly, from a Carthaginian point of ‖ view, the act of the Uticensians, in handing over their city to Rome, was a betrayal of the Punic cause. It is a pity, therefore, that Schweighäuser's objections to Reiske's suggested emendation, παραδιόντων (later improved by Bekker to παραδόντων), did not prevale, and this ill-judged correction has been admitted into standard modern editions.[31] So there is no support in the supposed Polybian parallels for an unusual translation of Thucydides' προδόντος. In the Thucydidean passage too emendation, to παραδόντος, was tentatively proposed by Classen in his first edition (Berlin, 1875). It is unfortunate that later editions promoted the emendation into the text, and that they were followed by the Loeb edition.

We must, therefore, translate προδόντος here with its normal meaning 'betray', and may render the whole passage as follows: 'When

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[27] For this interpretation of ἀναστάντες, see, e.g., E. F. Poppo, Thucydidis de bello peloponnesiaco libri octo, Part III.4 (Leipzig 1838) 17f.; Thukydides, erklärt von J. Classen and J. Steup, *ad loc.*

[28] Liddell and Scott cite two passages in papyri (respectively of the third century BC and fourth century AD) for the meaning 'to deliver up', a neutral sense, without the normal overtones of moral disapproval. But those feelings are universally present in the literary uses.

[29] See the very good explanation of this passage by Walbank, Commentary *ad loc.* Compare also προδόσεις in Ps.Dem. 50, Against Polycles, 7, as translated by J. S. Morrison, JHS 104 (1984) 51-2.

[30] See Poppo, *loc. cit.* who followed Schweighäuser in the lexicon of Polybius appended to his great edition (Leipzig 1789-95). Note that Poppo's and Schweighäuser's numbering of these passages is different from that of modern editions. What was 32.13.5 is now 31.27.5; what was 36.1.1 is now 36.3.1.

[31] E.g. T. Büttner-Wobst (Leipzig 1892-1924); W. R. Paton (Loeb, London 1922-7).

they had been expelled from Thapsus, they colonized Megara, which is called Hyblaea, because the Sicel king Hyblon betrayed the land to them and showed them the way.[32]

This examination of the literary tradition about the colonization of Megara Hyblaea allows a number of conclusions. It is certain that the colonists had great difficulty in finding a permanent site to settle. It is possible that they tried to land on territory already claimed by the Chalcidians at Leontini, and they certainly needed to join those Chalcidians in that colony. It is perfectly possible that the Megarians were perfidiously used to drive out the Sicels, with whom the Chalcidians had agreed on oath to live together at Leontini. It is certain that the Megarians were then driven out of Leontini by the Chalcidians, and that they found temporary refuge on the small peninsula of Thapsus. Bernabò Brea has well pointed out how easily defensible this position was against any attacks by land. Its very narrow isthmus could easily be blocked and held by a few determined men against attacks even of greatly superior numbers.[33] We may regard it as very likely, therefore, that the Megarians were expelled from Thapsus by a power that could attack them by sea, and that surely means by their fellow Greeks.

Finally, they were given a permanent site by the Sicel king, Hyblon. We are not told where he ruled, but his action in giving land to Greek colonists could be regarded by Thucydides as a betrayal. This judgment may imply that || Thucydides knew further details, which he decided not to pass on.[34] It would be possible to hypothesize, for example, that Hyblon belonged to some larger Sicel alliance against the Greeks, which he betrayed by giving land to the Megarian colonists. Or that his betrayal was of his own people. However, I am not convinced that it is necessary to postulate some circumstances such as these in order to explain Thucydides' 'betrayal'. Sufficient reason for such a judgement emerges readily from a reading of the preceding part of Thucydides' account of the early Greek colonization of Sicily (6.3). In this, the colonization of Syracuse was achieved forcibly at the expense of the previous Sicel inhabitants, as was also

[32] I follow Poppo, *loc. cit.* in taking the genitive absolute causally, as explaining the name of the new city, though that interpretation is not absolutely necessary.

[33] *Op. cit.* (n. 10 above) 171.

[34] Cf. Dover's note on Thuc. 6.4.1; Gomme, Andrewes, Dover, Historical Commentary on Thucydides IV (Oxford 1970).

that of Leontini. Syracuse and Leontini were near neighbours of Megara Hyblaea. The contemporary hostility between Greeks and Sicels in the immediate vicinity is a sufficient justification for Hyblon's act to be seen as a betrayal of the common Sicel cause. The use of the word προδόντος shows that in Thucydides' opinion the Greek colonization in eastern Sicily was a hostile act against the Sicels.

So much for the immediate literary sources about Megara Hyblaea's foundation. The next step is to place the picture drawn from these sources in the wider historical and geographical context. Bernabò Brea attempted to do this, using the full range of literary and archaeological evidence then available, in his paper of 1968.[35] In one major respect this paper is clearly superseded, for it accepted the high dating of the foundation of Megara mentioned above, which put it before that of Syracuse. As we have seen, this chronology was erroneous and may now be regarded as an aberration. But his general attempt to reconstruct the political situation in this part of Sicily at the time of the earliest Greek colonization is still thought-provoking.

His theses are based on his attempted reconstruction of the indigenous world at the time of the arrival of the Greeks. According to this, there were two major native kingdoms in the region with which we are concerned. One was ruled by Hyblon, whose capital was the great hill site of Pantalica, and whose territory stretched eastwards to the coast at Syracuse, and included Thapsus and what was to be Megara Hyblaea. To the North Hyblon's kingdom was bounded by the territory of the other Sicel realm, called Xuthia by Bernabò Brea, which was an older name for the territory round Leontini.[36] The capital || of this kingdom was at the site which became the Greek city of Leontini, and its territory included some of the rich plain to the North. According to Bernabò Brea, the rivalry between these two hypothesized native powers played its part in the experiences of the Greek colonists. For example, the Sicels were willing to accept Chalcidian colonists in a joint settlement at Leontini, in order to strengthen themselves against Hyblon, and Hyblon needed to befriend the Megarians, in order to strengthen himself against the threat from

[35] See n. 10 above.

[36] Diod. 5.8.2 (possibly from Timaeus; see FGH 566 F 164). Steph. Byz. seems to be in error, when he calls it a city, even though he cites Philistus (FGH 556 F 18).

the Chalcidians at Leontini, who had used the Megarians to expel the Sicels from their colony.

The starting point for this reconstruction was the identification of Pantalica as Hyblon's capital and the theory that its ancient name was Hybla. For the apparent connection between the personal name Hyblon and the common Sicel place-name Hybla has led to assumption that, if we can identify the right Hybla, we shall have the seat of Hyblon's kingdom. Since the Ephoran tradition stated that Megara Hyblaea itself was previously called Hybla,[37] it was natural first to seek a Sicel site in the near vicinity of Megara Hyblaea. The hill-town of Melilli, which lies some five kilometres to the South West, has been a favoured candidate, but was convincingly dismissed by Bernabò Brea, because the archaeological evidence shows a site of a quite different period, the Middle Bronze Age.[38] Bernabò Brea himself enthusiastically embraced a bolder hypothesis, that Hybla should be the most commanding native site known from archaeology in the whole region, Pantalica. This suggestion was made by Villard—though never, it seems, published by him—and has received widespread acceptance.[39] Although it cannot be proved or disapproved, the hypothesis depends entirely on a priori assumptions and uncertain arguments from probability. In any case, the search for Hybla may itself be a misguided enterprise.

It is far from certain that Megara Hyblaea was named after a pre-existing Hybla. That is part of the Ephoran tradition, which we have found to be unreliable. Thucydides can be understood to state that it was the king's name and not a place-name that was commemorated in the name of the Greek colony.[40] It is not even essential that the personal name Hyblon was derived from the place-name Hybla. The possibility exists that both personal and place-name might have arisen separately from the name of the Sicel divinity, which, it has been suggested, may have been the reason why

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[37] Strabo 6.267; cf. Ps.-Scymnus 277. The literary evidence for the various Hyblae is assembled by Dunbabin, *op. cit.* 144-5.

[38] Bernabò Brea, *op. cit.* 163. The identification of a Hybla with Melilli is still accepted by D. Asheri, "La colonizzazione greca," La Sicilia Antica I.1, ed. E. Gabba and G. Vallet (Naples 1980) 89-142, at 111.

[39] See Bernabò Brea. Sicily before the Greeks (London 1957) 164. Cf. also, e.g. Schenk, Graf von Stauffenberg, *op. cit.* (n. 10 above) 128 and 328 n. 5; Asheri, *loc. cit.*

[40] See above p. 311 and n. 32.

numerous places || in Sicily were called Hybla.[41] In any case, since no Hybla can be identified with certainty in the relevant area, the search for Hyblon's seat is not advanced by the assumption that it must have been called Hybla.

For these reasons, a theory, which starts from the assumption that Villard's bold proposal is correct, does not seem at all solidly based. It seems more prudent to adopt a more modest approach, and begin by considering what Greek and native communities existed in the area, when the Megarians were trying to found their colony.

Thucydides' very precise chronology of the first Greek colonial foundations in Sicily has not been shaken, either by our greatly increased archaeological knowledge, or by theoretical arguments.[42] It is true that the intervals with which we are here concerned are too small to be measured archaeologically, but the general reliability of Thucydides' colonial chronology increases our confidence in its details. According to this chronology, Chalcidians founded Naxos in 734 BC, and Corinthians Syracuse in the following year. The colonists of Naxos made a second settlement at Leontini in 729, and third at Catane at about the same time. The foundation of Megara Hyblaea followed very closely, *c.* 728.[43]

This sequence shows us what other Greeks were on the scene at the time the Megarians were attempting to establish their colony. The first reasonable coastal site, Naxos, and the strongest position in eastern Sicily, Syracuse, were already occupied. In between, the Chalcidian colonists had also established settlements at the northern and southern edges of the largest and richest plain in the whole area.

In the period and region under consideration we know of various Sicel communities, among whom may be mentioned: those who are

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[41] Cf. Dunbabin, *op. cit.* 131. Although some modern scholars call this deity Hybla (e.g. Dunbabin, 176), and others Hyblaea (e.g. Asheri, *op. cit.* 138; Hepding, RE s.v. Hyblaia), the name is not known, as L. Pareti correctly stressed; see Studi Siciliani ed Italioti (Florence 1914, published 1920) 334f. The words of our sole literary reference, Paus. 5.23.6, Ὑβλαίας . . . θεοῦ, have often been translated "the goddess Hyblaea," as, e.g., by W. H. S. Jones (Loeb) and J. G. Frazer (Pausanias' Description of Greece, London 1898). But the translation "the Hyblaean goddess" is clearly equally possible, and may well be better in the context of Pausanias' description of the place Hybla Gereatis. The most concrete information that we possess about this divinity, apart from Pausanias, comes from her name and title, Venus Victrix Hyblensis, on a Roman inscription (CIL X 7013).

[42] See my discussion in CAH III^2.3, 89-91.

[43] Thuc. 6.3-5; Dunbabin, *op. cit.* 435-6.

expelled from Ortygia by the Corinthian colonists of Syracuse; those who were expelled from Leontini; those who continued to live, as we know from archaeological evidence, in the immediate neighbourhood of the Greek colony at Leontini; those at the great hill settlement of Pantalica; and finally, those who lived at a site a little to the North West of Megara Hyblaea, in the valley of the river Marcellino, || near Villasmundo.[44]

It is not possible, unfortunately, to establish what were the political groupings and interrelationships of these Sicel communities. No literary source describes them and on such subjects archaeology is mute. However, the last community mentioned, which lived about six kilometres away from Megara Hyblaea, inevitably attracts attention. Only a small selection of the finds have been published so far, and the investigations seem to have been largely confined to graves, so any attempts at far-reaching historical reconstructions would be inappropriate. Nevertheless, we have now a Sicel site in close proximity to Megara Hyblaea, which was certainly inhabited in the eighth century. Furthermore, the finds include pottery, which makes it probable that these peoples had contacts with the eastern Mediterranean, before any Greek colonization in Sicily took place.[45]

We certainly cannot know, and are perhaps never likely to know with certainty, whether the site near Villasmundo was Hyblon's royal seat. The position and extent of his kingdom is not described for us in any of our literary sources, and identifications with known archaeological sites, though tempting, are not susceptible of proof. Nor are we likely to achieve a certain reconstruction of the motives and actions of all those concerned with the colonization of Megara Hyblaea. Nevertheless, the facts that we do possess allow a simple and probable general scheme. The Megarian colonists were squeezed by Chalcidians at Naxos, Catane and Leontini, and by Corinthians at Syracuse. They were certainly expelled from Leontini by the Chalcidians, and may well have been expelled from Thapsus by the

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[44] See Thuc. 6.3.2 (Syracuse); 3.3. (Leontini); Polyaen. Strat. 5.5 (Leontini); CAH III^2.3, 104 (Leontini); Bernabò Brea, *loc. cit.* (Pantalica); G. Voza, "La necropoli della valle del Marcellino presso Villasmundo," Insediamenti coloniali greci etc. (n. 3 above) 104-10 (Villasmundo). This last site was in fact mentioned by Bernabò Brea in his article of 1968 (n. 10 above); see p. 164 with n. 16 and map, p. 165, where it is the site numbered 5.

[45] See Voza, *op. cit.*

Syracusans. Thapsus is a mere eleven kilometres from Syracuse.[46] Under pressure from their fellow Greeks, they sought friends among the native population. Hyblon knew that neighbouring Sicels had recently been expelled from their homelands by Greeks at Syracuse and Leontini. Whatever his relationship with those Sicels, which we cannot know, he would be uncomfortably aware that the Greeks possessed superiority in the military arts. It would be a sensible precaution for him to secure the help of some of those Greeks by offering them land for their colony.

This reconstruction, while it differs from Bernabò Brea's in many particulars, || follows his general contention, that it was mutual need and the prospect of mutual benefits that led the native king to make his offer and the Greeks to accept. We may be sure that such motives of self-interest were important. On the other hand, we should not exclude the possibility that friendly relations existed for their own sake. It is an interesting new fact that the only site yet known in Sicily, where evidence have been found of probable pre-colonial contacts, was a mere six kilometres from the land given to the Greek colonists, and almost certainly formed part of Hyblon's kingdom. These people had previously been open to overseas traders from the eastern Mediterranean, and were already familiar with Greek products. Such knowledge and experience may have played its part, in addition to careful calculations of advantage, in causing our first clear instance of colonization by invitation. ||

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[46] The significance of Thucydides' word ἀναστάντες is noted above. Cf. Bernabò Brea, *op. cit.* 175 (though with the wrong conclusion: that the Greek colony at Syracuse did not yet exist).

10. THE FOUNDATION OF THASOS*

Thasos must be one of the best-known, archaeologically, of all Greek colonies, so it is something of a paradox that the material evidence for its foundation and earliest days is far from satisfactory. We have no early graves,[1] and, until 1960, there was no evidence from any very early settlement either. True, in more than one sanctuary it is assumed that the finds go back (in general terms) to the beginning of the life of the Parian colony, but none of this material is demonstrably earlier than *c.* 650,[2] a time which is very generally thought to be about a generation later than the foundation of the colony.[3] So even quite recently it was possible to write "It seems likely that it was founded from Paros by about 680 BC. . . . The earliest pottery which can be dated is Corinthian and Rhodian, none of it much earlier than the mid seventh century."[4]

The two deep soundings of 1960 and 1961 provided us for the first time with excavated material from Thasos definitely earlier than *c.* 650.[5] Small though it is in quantity, this evidence transforms our picture of early Thasos. Its correct interpretation is an essential part of any reconstruction of the historical circumstances of the foundation of the colony. This is my chief reason for offering a

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[1] L. Ghali-Kahil, *Études Thasiennes* VII, *La Céramique Grecque* (Paris, 1960) 11. By contrast we have considerable knowledge of later cemeteries; see *BCH* lxxviii (1954) 225-51.

[2] For the sanctuaries and dates see P. Bernard, *BCH* lxxxviii (1964) 77; cf. J. Pouilloux, *Études Thasiennes* III, *Recherches sur l'Histoire et les cultes de Thasos* (Paris, 1954) 14f.; Id. "Archiloque et Thasos: histoire et poésie," *Fond. Hardt* x (1963) 14; J. Boardman, *The Greeks Overseas*[2] (London, 1973) 225.

[3] See, for example, Pouilloux, *Et. Thas.* III, 22ff.; *Fond. Hardt* x. 22; Bernard, *op. cit.* 142; Boardman, *ibid.*; and my remark in *JHS* xci (1971) 36. L. H. Jeffery puts Thasos *c.* 700 in *Archaic Greece* (London, 1976).

[4] Boardman, *ibid.*

[5] P. Bernard, "Céramiques de la première moitié du VIIe siècle à Thasos," *BCH* lxxxviii (1964) 77-146; hereinafter I refer to this paper by the author's name alone.

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reconsideration of a subject which has been much discussed;[6] but there are subsidiary reasons. The pre-Greek inhabitants of the island have long been almost invisible archaeologically. Recently, however, good evidence for prehistoric settlements has been found at two sites in the southern and western parts of the island.[7] There is also important archaeological evidence for Thasian settlements on the mainland which is valuable for our understanding of the colonization of Thasos.[8] Finally, important new light has been cast on the role of the Phoenicians in the early history of the island. Although attested by Herodotus (ii. 44. 3-4; vi. 47), this has been very generally played down or even entirely discounted,[9] because of the presumed absence of archaeological or other confirmation. It seems to me that the works of Salviat and Servais[10] and of van Berchem[11] have now firmly established that there is a solid historical reality behind Herodotus' statements. We thus have a number of new factors which we must take into account in any attempt to recover the story of the Parian colonization of Thasos. ||

Archaeological evidence

The two sondages of the Champ Héraclis (I) and the Champ Dimitriadis (II) have been published *in extenso* by Bernard,[12] but a brief description and analysis is required here. I lies in the general area of the city east of the Agora, between the Artemision and the Dionysion, to the south of the road which goes through the Passage des Theores and then passes the Dionysion on its way towards the

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[6] Pouilloux, *Et. Thas.* III, 22ff., includes abundant references to the earlier literature (cf. also his later treatment, *Fond. Hardt* x. 3ff.). The ancient evidence was usefully collected by E. Jacobs, *Thasiaca* (Berlin, 1893, 6-12, and by C. Fredrich, *IG* xii. 8 pp. 74ff. F. Jacoby, "The date of Archilochus", *CQ* xxxv (1941) 97-109, is the most important treatment of the chronology.

[7] See H. Koukouli-Chrysanthaki, *Arch. Eph.* 1970 (Chron.) 16-22, and *AAA* iii (1970) 215-22. For the positions of the sites see D. Lazaridis, *Thasos and its Peraia* (Athens, 1971) figs. 20, 23, 26, etc.

[8] *Arch. Delt.* (Chron.) xx (1965) 447-51; cf. xxiv (1969) 349-51; *AAA* vi (1973) 230-40.

[9] See the list of sceptics assembled by F. Salviat and J. Servais, *BCH* lxxxviii (1964) 282f.; cf. also Boardman, *op. cit.* 224.

[10] "Stèle indicatrice Thasienne trouvée au sanctuaire d'Aliki," *BCH* lxxxviii (1964) 267-87, especially 278-84.

[11] "Sanctuaires d'Hercule-Melqart," *Syria* xliv (1967) 73-109, 307-36.

[12] See n. 5 above; I use Bernard's terminology.

theatre (see fig. 5).[13] The sondage itself is some 50 m. SE. of the Dionysion.

Its position is thus on a direct line between the Acropolis and the War Harbour, some 150 m. inland from the latter. In this part of the town the coastal plain rises gradually SE. by terraces towards the steep northern face of the Acropolis.[14] The excavation has revealed a residential area going back to the early archaic period.

Sondage II was effected by re-opening the extreme eastern end of the previously excavated area in Champ Dimitriadis, which lies NE. of the Poseideion, underneath the slopes crowned by || the theatre.[15] Here the coastal plain is narrower. The position of II is about 100 m. inland from the sea in the Commercial Harbour. The Champ Dimitriadis region has long been known to have been a residential area with a very long history, stretching back to the early archaic period. Sondage II is some 200 m. NE. of I. However, they are both in the same general area of the city, and they are both on the same contour line, i.e. where the first slopes of the Acropolis begin, a zone especially well-supplied with water.[16]

The lowest levels in II were much damaged by later buildings. However, close to the rock a thin layer of earth containing similar pottery to that found in I offered a few undisturbed pockets to the excavators.[17] Two parallel fragmentary walls were also attributable to a building of this earliest period.[18] For really informative stratigraphy, however, we have to turn to I (figs. 6 and 7). Here too, the sounding was taken down to the rock (a depth of *c.* 7 m.).[19] Beneath a room dated to the last quarter of the seventh century by an early Corinthian aryballos and 'Melian' sherds, four levels were distinguished, named from top to bottom Z, Y, X, W. Of these two, i.e. W and Y, were identified by the excavators as occupation levels, which were both ended by fire. X and Z consisted of clean sandy

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[13] In addition to the general plan of Thasos and the excavations, reproduced from *Guide de Thasos* (Paris, 1968) fig. 4, see the plan in *BCH* lxxxv (1961) fig. 1, opp. p. 918.

[14] Bernard, 78; Daux, *BCH* lxxxv (1961) 931-5.

[15] Bernard, 83. Compare his fig. 3 with *BCH* lxxviii (1954) fig. 8, opp. p. 192. For the general position of the Champ Dimitriadis section, see the general plan fig. 1 (n. 13 above) and *BCH* lxxxvi (1962) fig. 1 p. 935.

[16] As Bernard, 85.

[17] Bernard, 83-6; *BCH* lxxxvi (1962) 938.

[18] Bernard, 86, and figs. 3 and 4 (walls *e* and *f*).

[19] *BCH* lxxxv (1961) 935.

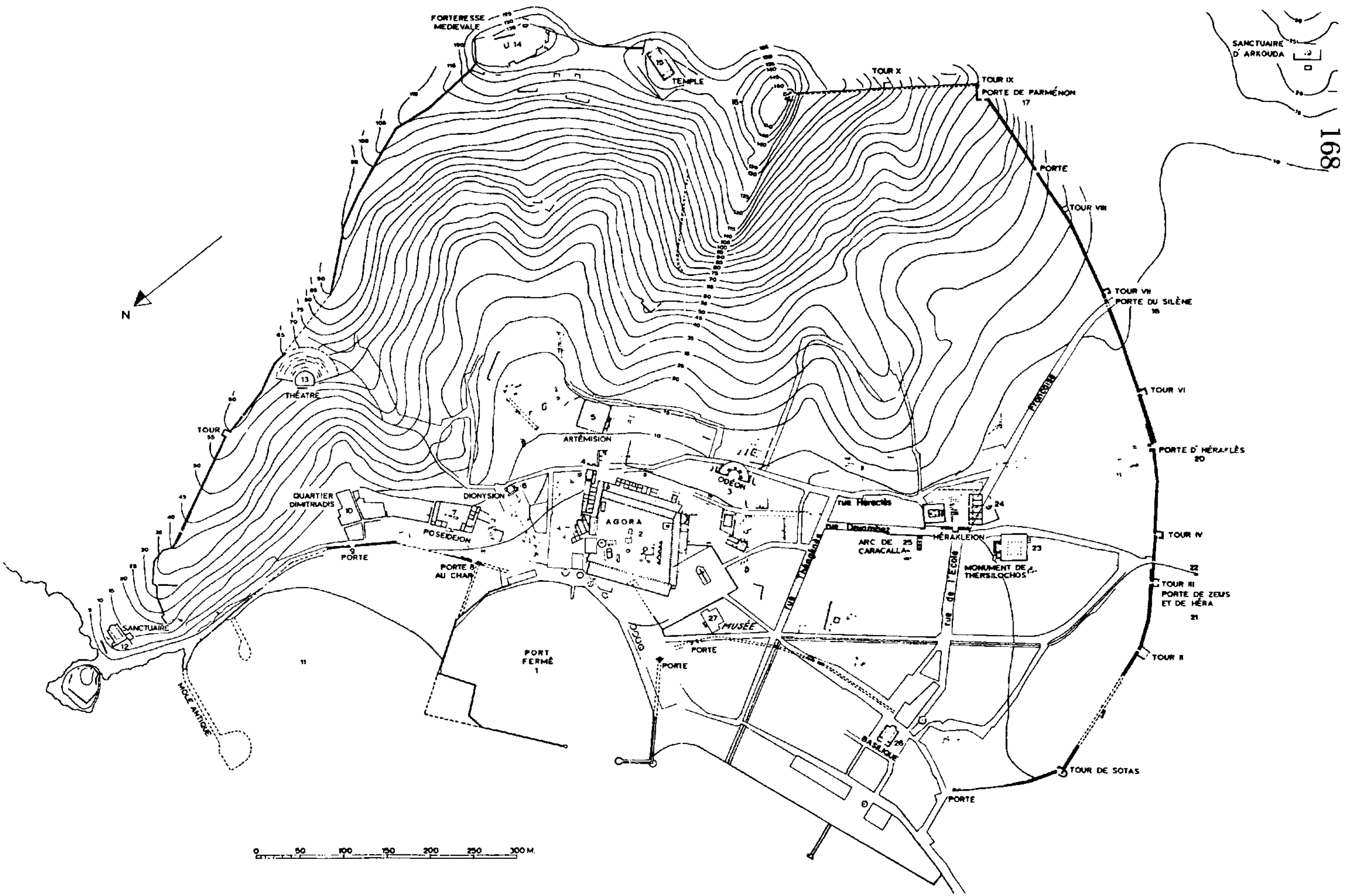

Figure 5. Thasos. General plan of city and excavations (*Reproduced by kind permission of the École Française d'Athènes.*)

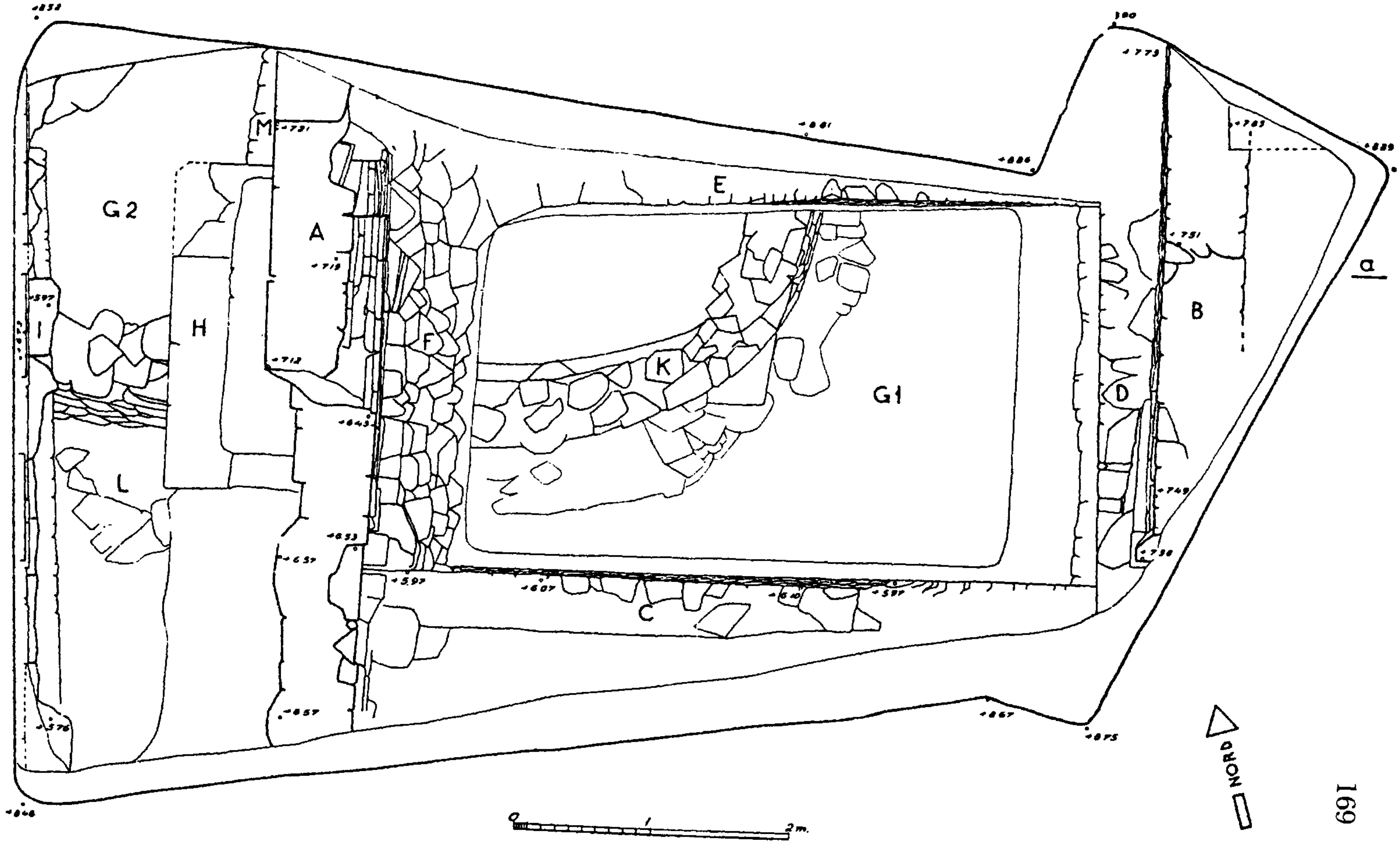

Figure 6. Sondage I (Champ d'Héraclis). Plan (*Reproduced by kind permission of the École Française d'Athènes.*)

soil containing very many shells, and were || interpreted by the excavator not as occupation levels but as fill brought to cover the occupation and burnt levels below them.[20]

The bottom level W was associated with an apsidal (or ellipsoidal) house, of which parts were preserved in the area excavated, and the burning which terminates the level is presumed to have destroyed the house. This bottom level (W) was in total *c.* 1.60 m. thick, a little more than the combined depth of the three levels above (X, Y, Z). Its lower part (*c.* 1.20 m. in depth) consisted of blackish soil (clay with a large admixture of carbon and ashes which gave it its colour) irregularly divided by layers of sand packed with shells. It was tentatively suggested[21] that this might have been the result of burning scraps and then scattering sand on top. It is interesting to note that some of the thin undisturbed early level in II showed similar characteristics.[22] The upper part of W is the destruction level proper (*c.* 0.45 m.), which the excavators found easy to distinguish by its homogeneous character from the occupation level below it.[23]

Although the depth of W was thought to imply a relatively long occupation, the pottery in it was not thought to show any development. The sherds from the destruction stratum at the top were not different from those further down.[24] Questions arise about the excavator's interpretation || of his finds in W. The drawn section shows that the blackish soil we have just described was the same whether it was outside the house (wall K) or inside it beneath the indicated floor (fig. 7). This floor (consisting of a layer of reddish clay) relates well with the house wall, and stones fallen from the walls were found upon it.[25] But no other floor was apparently recognized beneath it. In view of the similarity of the soil outside the house and underneath the floor inside, a possible interpretation would seem to be that this soil represents deposits of rubbish, on which the house was built. The character of this soil did in detail suggest deposits of rubbish to the excavator, as we have seen. If this is correct, W offers a clear picture of a house with one floor level built on rubbish

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[20] Bernard, 87 and fig. 2.

[21] Bernard, 87.

[22] Bernard, 85: "deux couches très minces d'une terre noirâtre mêlée de charbons, séparées par une couche de terre sablonneuse."

[23] Bernard, 87.

[24] *Ibid.*

[25] Bernard, fig. 2 and 81f.

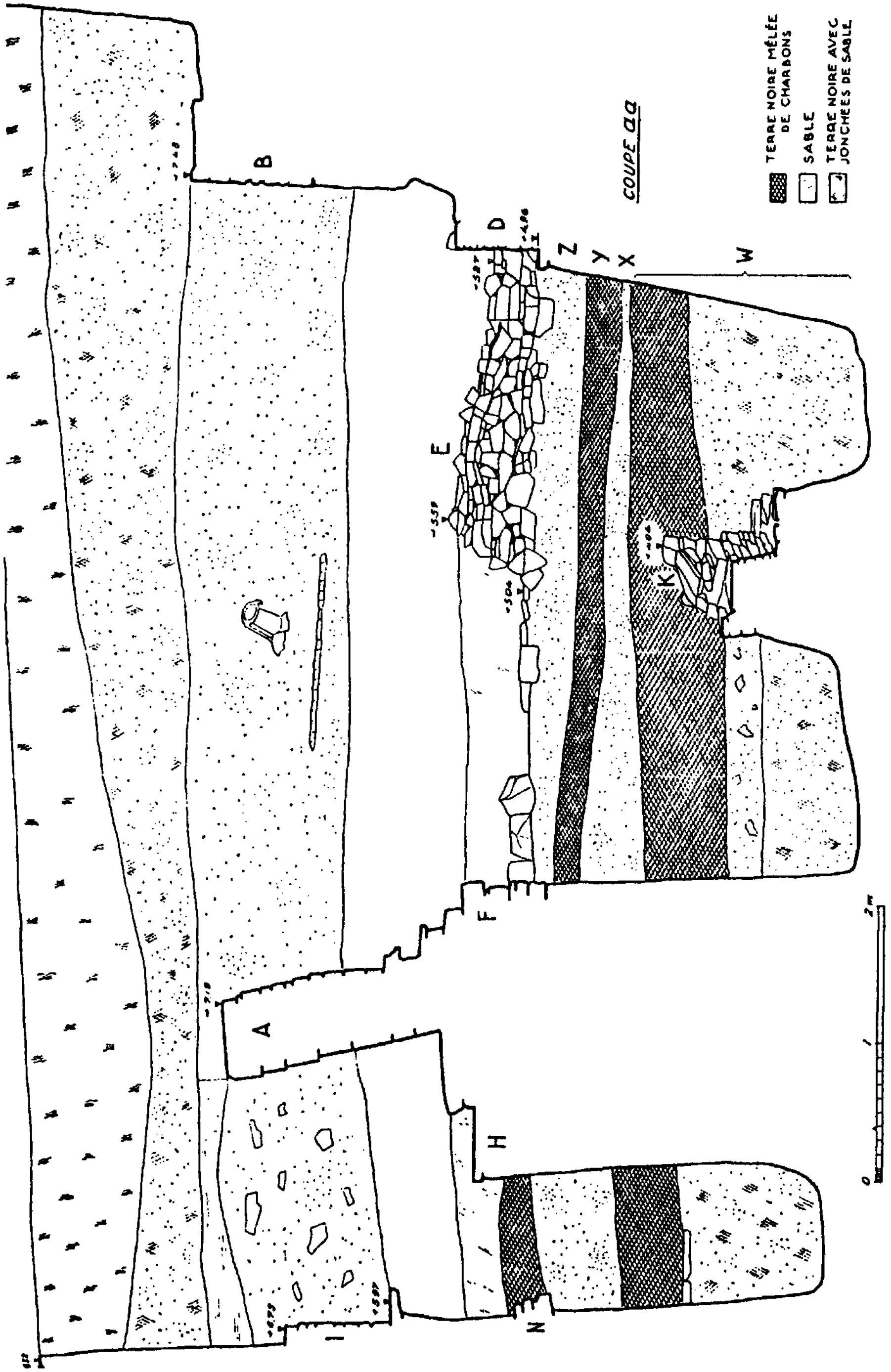

Figure 7. Sondage I (Champ d'Héraclis). Section. (*Reproduced by kind permission of the École Française d'Athènes.*)

deposits, which was destroyed by fire. Since we cannot tell how quickly such rubbish deposits were built up, no conclusions can be drawn for chronology from the depth of the level W. The homogeneity of the pottery found within it seems to be the only guide.

There are also questions about the higher occupation level recognized, Y. Here the section shows only the burnt soil which in W was thought, no doubt rightly, to represent the actual stratum of destruction.[26] Furthermore, while we have a house and a floor in W, nothing was found in Y which seems to prove occupation. Finally, while the top level Z is easily accepted as a levelling fill for the building above it, X seems too uneven to represent such levelling, which increases one's doubts about Y's claim to be an occupation level. No doubt such questions could only be resolved by opening up a larger area.

On the basis of his interpretation of the stratigraphy Bernard drew the following chronological conclusions.[27] An 'Ionian' amphora found in the top level Z is datable to *c.* 625. Some time is allowed to Y, interpreted as we have seen as an occupation level. Thus W is thought to lie *grosso modo* in the first half of the seventh century. In view of what we have said about the stratigraphy it will be clear that chronological conclusions founded upon it are risky. However, Bernard claimed that they were in agreement with the chronological indications of the pottery and the apsidal house.

Of the house part of the apse at the east end and two portions of the side wall on the south were revealed. These walls were of irregular stone construction, *c.* 0.40 m. thick; the greatest height preserved was 0.80 m.[28] Also found was what may be part of a cross-wall. Although it was not possible to test this by digging, the fact that the levels in G2 are higher than in G1 strongly suggests that there was some barrier between them, i.e. a cross-wall[29] (figs. 6 and 7). Since much of the house was outside the limits of the trench we cannot tell if its west end was curved (i.e. an ellipsoidal plan) or straight. Fragments of clay bearing the impressions of reeds found in the ruins of the house[30] were interpreted by the excavator as

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[26] Bernard, fig. 2.
[27] P. 87.
[28] Bernard, 80f. and fig. 1.
[29] *Ibid.* and fig. 2.
[30] Bernard, 82 and fig. 6.

belonging to the roof, rather than (the other possibility) to the upper parts of the walls. The preserved height of the stone walls discovered certainly suggests that they were more than socles.

Bernard adduced as analogies for his house many examples from the Greek world generally, and particularly houses from Old Smyrna which he thought to be late Geometric.[31] But the analogy is not apt. In the first place all the early houses at Old Smyrna were of mudbrick,[32] and there is no sign of mudbrick in the debris of Thasos, as the excavator describes it. Furthermore, the house at Old Smyrna with which Bernard compared his Thasian house is not late Geometric, but Protogeometric.[33] In fact, the primitive apsidal structures at Old Smyrna are || scarcely found after 700 BC.[34] Bernard thus drew faulty chronological conclusions from the analogies he adduced, when he thought that the houses at Old Smyrna confirmed a date in the first half of the seventh century for the apsidal house. In any case, such a primitive building as this house[35] is not in itself closely datable, and Bernard himself clearly recognized the primacy of the ceramic evidence in establishing his chronology. So we may turn to the pottery found in the two sondages, which was perhaps surprisingly varied and suggestive to emerge from such relatively restricted excavations.

Bernard divided the local ware into two categories, the coarse hand-made ware[36] and the local ware of Macedonian type.[37] Of the former the only type worth mentioning seems to be the tripod pot used for cooking,[38] because it was not previously attested on Thasos. The disadvantages of the shape from a practical point of view were noted by Sparkes,[39] but its range in both place and time "Bronze

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[31] Bernard, 82f.

[32] These important early houses at Old Smyrna have not yet been properly published. Brief descriptions, photographs, and reconstructions drawn by R. Nicholls, are to be found in E. Akurgal, *Die Kunst Anatoliens* (Berlin, 1961) 9ff., 301. Cf. also the briefer accounts in *AJA* lxvi (1962) 369ff., and H. Drerup, *Griech. Baukunst in geometrischer Zeit* (*Arch. Homerica* II ch. O; Göttingen, 1969) 44-7.

[33] In his review of Drerup (*Gnomon* xliv [1972] 698-704) Nicholls has pointed out that the figures on p. 301 of Akurgal, *op. cit.*, are miscaptioned. Fig. 3 is a rectangular house (*not* apsidal); fig. 4 is the same protogeometric oval house illustrated in fig. 1 (and p. 9 with Abb. 1 = *AJA* lxvi [1962] pl. 96 fig. 3).

[34] Nicholls, p. 700.

[35] Compare Salviat's description, 'ces huttes assez primitives'; *Actes du VIIIe Congrès Internat. d'Archéologie Classique*, Paris, 1963 (1965) 302.

[36] Pp. 131-6.

[37] Pp. 126-31.

[38] See p. 134.

[39] *JHS* lxxxii (1962) 130.

Age to Hellenistic)[40] is too wide to provide very useful information. Since Bernard wrote similar hand-made pottery has been discovered on the adjacent mainland (below p. 87).

The local ware of Macedonian type consists of oenochoae with cut-away necks and drinking cups; some of this pottery was found in the lowest levels.[41] The oenochoae represent one of the most characteristic types of Macedonian pottery in the Iron Age,[42] but the shape is extremely long-lived and parallels are known which extend in time from the tenth to the fourth centuries BC.[43] The drinking cups are best paralleled by finds from pre-Persian Olynthus, but, unfortunately, all Olynthian material before 479 was effectively dated anywhere in the Iron Age earlier than that date.[44] Bernard noted that the two types are not only Macedonian in their shapes; they are similar, too, in their fabric and in the occasional employment of very sober incised decoration. In view of this I do not see why it is necessary to conclude that the Thasian material represents local production closely related to Macedonian potteries;[45] unless we are bound to assume that all hand-made pottery must be locally produced. But this is rather superior hand-made ware, and, since we know that some of the pottery from these excavations was imported from the Macedonian region (see *infra*), it is surely a more economical hypothesis to assign this ware also to the imported category.[46]

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[40] *Ibid.*

[41] E.g. nos. 163, 165 (p. 128).

[42] Bernard, 126. See, for example, the excellent discussion of the very numerous examples from the great cemetery at Vergina; M. Andronikos. *Vergina* I (Athens, 1969) 194ff.

[43] Bernard, 126; David M. Robinson, *Excavations at Olynthus* V (Baltimore and London, 1933) 49. The obstacles in the way of establishing the typology and chronology of these jugs are well emphasized by Andronikos, *ibid.* The brave attempt by Hammond to organize the chronology of Early Iron Age Macedonia (*History of Macedonia* I [Oxford, 19-2] ch. xv) shows its great difficulties and complexity, and offers little for the pottery.

[44] Bernard, 130; *Olynthus* V pls. 23, 24; for the chronology, pp. 15ff., 47-63, and XIII 3ff. Hammond assumes that Iron Age Olynthus begins *c.* 650; *op. cit.* 358f. He uses the literary evidence about the expulsion of the Bottiaeans from Macedonia (Thuc. II. 99. 3, 100. 2; cf. Hdt. VIII 139). While that may give us a (very approximate) date for the arrival of the Bottiaeans in the area, it is clear that the excavators did not envisage that arrival as the beginning of Iron Age settlement at Olynthus, which they put long before; see *Olynthus* XIII, 2. On the evidence that they found, the Bottiaean arrival offers no archaeological date for material at Olynthus.

[45] As Bernard, 126.

[46] Cf. Salviat, *Actes du VIIIe Congrès*, etc. 302: "Il n'est pas exclu que certains de ces vases aient été effectivement importés de Macédoine."

Of the pottery judged by Bernard to be imported we may appropriately begin with the painted ware of Olynthus. Although only a small number of sherds was found, there is no doubt about their identification with the Group I, Painted Ware, of Olynthus.[47] It is, once again, most || unfortunate that this group can only be dated very approximately to before 479. Since the related Macedonian painted pottery is attested as early as *c.* 1050, the *terminus post quem* must theoretically be as high as the eleventh century.[48] These sherds are, therefore, of no great value in establishing chronology, but, in this instance at least, there is no doubt that we have pottery brought from the Macedonian/Chalcidian area.

The remaining imported ware of which there was any quantity came, it seems, from the east and south. The Grey Bucchero and Grey Ware with Black Glaze are both taken to represent imports from Asia Minor, the former more particularly from the Aeolian area. (It is possible, however, that some of the sherds of lower quality are locally made copies.)[49] Grey Ware has a very great geographical range and was produced over a long period, but we may confine ourselves here to noting its presence in large quantities in Troy VIII, on Lemnos and on Lesbos.[50] The finds at Troy were dated to the general period seventh and sixth centuries,[51] but the beginning of Troy VIII was set by the Cincinnati excavators rather too low at *c.* 700,[52] so we should rather write eighth to sixth centuries. It is significant that very little Grey Ware had been discovered at Thasos before the excavations under discussion, and the few fragments known also came from the Champ Dimitriadis.[53]

The amphorae with geometric decoration, of which numerous fragments were found (in all the levels W, X, Y, Z, but especially in the lowest, W),[54] are best paralleled by finds from Troy VIII.[55] The

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[47] Bernard, 124; *Olynthus* V pl. 22, pp. 23-4.

[48] Bernard, 124; *Olynthus* V p. 50.

[49] Bernard, 109-15.

[50] Bernard provides a good discussion and bibliography, *ibid.*

[51] C. Blegen, C. G. Boulter, J. L. Caskey, Marion Rawson. *Troy* IV (Princeton, 1958) 253.

[52] Cf. J. N. Coldstream, *Greek Geometric Pottery* (London, 1968) 376; J. M. Cook, *Troad* (Oxford, 1973) 101, citing the Oxford thesis of N. P. Bayne, who thinks that "the grey ware and sparse geometric (at Troy) would go back a good deal earlier than 700 BC."

[53] *Et. Thas.* VII. 45f.

[54] Bernard, 116-23.

[55] *Troy* IV fig. 302, 2a-2d (p. 279); fig. 310, 21, 22-4 (p. 296).

dating of the finds at Troy is not precise;[56] we can only use again the beginning of Troy VIII as an approximate *terminus post quem*. The place of manufacture of these vessels is not certainly known, but a site in Asia Minor is thought likely.[57] Presumably we have in these large containers evidence for the import of wine.

None of the wares so far discussed provides close dating. The attractive subgeometric painted pottery, the so-called G 2-3 Ware, of which large quantities were found,[58] is, therefore, all the more important. G 2-3 Ware proper, or, as Bernard calls it, 'Aeolian G 2-3 Ware', is named from the square at Troy where it was found in abundance.[59] However, there is also a Lemnian variety, found in large quantities at Hephaestia, 'Lemnian G 2-3 Ware', as Bernard called it, which he convincingly shows to be very closely related to Aeolian G 2-3 Ware.[60] This Lemnian variety was certainly manufactured on Lemnos.[61] It is generally slightly inferior to Aeolian G 2-3 Ware, so Bernard suggests that we should distinguish Lemnian from Aeolian G 2-3 Ware and regard the Lemnian as a local imitation.[62] Examples of such Lemnian G 2-3 Ware were also found at Troy, though they were not there distinguished from Aeolian G 2-3 Ware.[63] The parallels for the material found at Thasos are entirely convincing, and we may follow Bernard in his suggestion that this pottery was imported from Lemnos.[64]

The dating of the Lemnian G 2-3 vases depends on the general dating of the cemetery at Hephaestia. The lower terminus of this cemetery is satisfactorily established as in the second half of the seventh century.[65] Nothing precisely fixes the upper bracket, but Mustilli argued that it could not be higher than the eighth century because of the "carattere unitario che la necropoli presenta."[66]

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[56] Cf. *Troy* IV 273, 294.

[57] As suggested already by L. Ghali-Kahil, *Et. Thas.* VII 33; also Bernard, 116.

[58] Bernard, 88-105. At least one fragment of this ware was found earlier on Thasos; see *Et. Thas.* VII plate xxiii, 50 (cf. Bernard, 98f.).

[59] *Troy* IV 253-5.

[60] Bernard, 88-90.

[61] Bernard, 90.

[62] *Ibid.*

[63] See Bernard, 106. On the dating of the G 2-3 Ware from Troy, see below p. 68.

[64] Bernard, 105-9. A small number of sherds of this Lemnian pottery were found before the excavations of 1960 and 1961; see *Et. Thas.* VII, 41f., 1-3 (pl. xiv 1-3); cf. Bernard, 107-9; and some more were found in excavations in the Champ Dimitriadis in 1962; see *BCH* lxxxvii (1963) 846.

[65] D. Mustilli, 'La necropoli tirrenica di Efestia', *ASA* xv-xvi (1932-3) 243, 222-8.

[66] *Op. cit.* 243.

The place of manufacture of Aeolian G 2-3 Ware is not known. The pottery has been found on Lesbos, Lemnos, Samothrace, Thasos, and at Troy, Settlement VIII. Bernard provides much the fullest account of the pottery available; he describes its character, sets out the evidence and arguments about its provenance, and discusses its chronology in detail.[67] Since we are trying to date the Thasian material, we must naturally concentrate on the chronology of the other sites.

The G 2-3 Ware from Antissa on Lesbos was found in the two apsidal buildings excavated by Miss W. Lamb.[68] The excavator seems to have regarded the sherds as 'geometric'[69] and implied that most of the material came from the earlier building, which she thought was erected in the ninth or tenth centuries and survived into the eighth.[70] However, since the second of these buildings probably survived into the seventh century,[71] and the stratigraphical context of the sherds cannot be distinguished with complete certainty,[72] we must agree with the critics[73] that the date of this pottery is correctly to be described as seventh century or earlier.

The Aeolian G 2-3 Ware from Lemnos is all unpublished,[74] and there is no indication of the precise contexts in which it was found. We can do no more than note that it was found in some quantity in the excavation of the settlement at Hephaestia and in the Sanctuary of the Cabiri at Chlöi.[75]

There is no strict *terminus ante quem* or *terminus post quem* stratigraphically for the G 2-3 Ware of Troy, Settlement VIII, but none of it was found in areas which yielded much East Greek and Corinthian pottery of the second half of the seventh and early sixth centuries. The pottery was thus thought to lie between the beginning of Troy VIII and the second half of the seventh century.[76] Since it is quite

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[67] Pp. 88-105.

[68] *BSA* xxxii (1931-2) 41-67; see her plate 23 and the comments on p. 254 of *Troy* IV.

[69] *Op. cit.* 56.

[70] *Op. cit.* 45.

[71] *Op. cit.* 47.

[72] Cf. the 'percolation' of sherds mentioned on p. 45.

[73] *Troy* IV 254; Bernard, 93.

[74] See Bernard, 88.

[75] *Ibid.* For brief general accounts of these excavations, see *ASA* x-xii (1927-9) 712f.; xii-xiv (1930-1) 499f.; n.s. i-ii (1939-40) 223f.; cf. *Arch. Eph.* 1937, 629-54.

[76] On the stratigraphy see *Troy* III 158; IV 280. The arguments on the date of the ware are well set out at IV 254 (cf. 249f.), though the authors' ignorance of

unlike the local Phrygian pottery, the Cincinnati expedition took it to be among the earliest Hellenic wares of Greek Troy. The style of the decorative motifs was regarded by them as too late and developed to allow a date early in the eighth century.[77] They were thus inclined to follow the date suggested by Lehmann for his Samothracian material (see *infra*) and date G 2-3 Ware tentatively to the first half of the seventh century. Without the Samothracian analogy it is clear that a rather wider dating would be possible from the evidence of Troy itself.[78]

The finds of G 2-3 Ware at Samothrace are, therefore, of great importance. Unfortunately, our information is limited to the preliminary report.[79] In this Lehmann described the discovery of a stratum of sacrificial debris consisting of burnt bones and pottery fragments at a low level in the NW. part of the temenos.[80] He wrote 'The fill is homogeneous, subgeometric of the first half of the seventh century BC.[81] It was on the basis of these discoveries that Lehmann dated the || arrival of the Greeks on Samothrace to *c.* 700 BC.[82] Since they are so significant both for the history of Samothrace and for our knowledge of G 2-3 Ware, it is most regrettable that no clear evidence for the dating of these finds was presented by Lehmann in the preliminary publication. The pottery described and illustrated consists either of G 2-3 Ware or of local hand-made pots, neither of which can be called chronologically fixed. And it is obvious from the character of the area in which the finds were made that dating by stratigraphy is impossible.[83] Perhaps all doubts would be laid to rest if we knew about the "other fragments of subgeometric from this deposit" which "point to importation from Rhodes and the

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the Lemnian material is remarkable (cf. Bernard, 89, who, perhaps charitably, assumed that they were consciously excluding it from discussion).

[77] Professor R. M. Cook, who kindly discussed this pottery with me, thought it should be of the seventh century by style.

[78] On the date of the beginning of *Troy* VIII, see above p. 67.

[79] *Hesp.* xxi (1952) 20, 33-7, especially 34f.; plates 3-10.

[80] See K. Lehmann, *Samothrace, A guide*[4] *etc.* (New York, 1975) 61f.; cf. the plan of the site at end. In *Hesp.* xxi the area was called "the precinct on the Central Terrace."

[81] *Hesp.* xxi (1952) 35.

[82] *Ibid.* From *Guide*,[4] 15 and P. W. Lehmann, *Skopas in Samothrace* (Northampton, Mass., 1973) 2f., it appears that this is still accepted doctrine.

[83] The area has been dug officially twice, and has been much damaged by natural erosion, illicit digging, and agricultural operations. All this in addition to repeated rearrangement in antiquity. See Lehmann, *Hesp.* xxi 19-21 and plates 3-8.

Cycladic islands."[84] But in default of adequate information about these sherds, I at least am somewhat uneasy about basing our chronology of G 2-3 Ware on the Samothracian material.

Apart from Lehmann's confidently stated date the Aeolian G 2-3 Ware must be dated by the finds on Lemnos, Lesbos, and at Troy. At the first site the only indication seems to be in the approximate dating of the Lemnian G 2-3 Ware from the cemetery at Hephaestia (to which the Aeolian G 2-3 Ware is very closely linked), i.e. before the end of the seventh century but not earlier than the eighth. The finds at Antissa can only be placed in the seventh century or earlier, and those at Troy between some date in the eighth century and the second half of the seventh. The closer date, first half of the seventh century, is dependent on the correctness of the date of the material at Samothrace, though it may be supported by considerations of style.

One sherd remains which may have a bearing on the chronology of the deep soundings on Thasos, a rim fragment from one of the famous skyphoi with pendent concentric semicircles.[85] This was found in level Y, which Bernard would put *c.* 650 on his chronology, but he suggests that it may be an isolated sherd which has risen to a higher level. A serious conflict between this sherd and Bernard's dating only arises if we accept a high chronology for the skyphoi with pendent concentric semicircles.[86] Such a high chronology was thought in the past to be indicated by the absence of such skyphoi from the West. There are now a few examples from western sites, but it is still true that none has been found in a Greek colonial context. An

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[84] *Op. cit.* 37.

[85] Bernard, 140, fig. 52 (no. 220); see also 142.

[86] These famous cups are discussed by, among others, V. Desborough, *Protogeometric Pottery* (Oxford, 1952) 180-94; G. M. A. Hanfmann, *The Aegean and the Near East: Studies presented to Hetty Goldman* (New York, 1956) 173-5; and J. N. Coldstream, *Greek Geometric Pottery* (London, 1968) 156f., 310f. H. G. Buchholz, *Berl. Jahr. f. Vor- und Frühgesch.* v (1965) 224-31, provided a comprehensive list (with bibliography) of finds to that date, and P. J. Riis' useful discussion in *Sukas* I (Copenhagen, 1970) is particularly valuable for their distribution in the East. For the chronological conclusions drawn from their supposed absence from the West, see especially J. Boardman, *JHS* lxxxv (1965) 12 n. 27; cf. *Greek Emporio* (*BSA* Suppl. 6; London, 1967) 117, and V. Desborough, *AA* 1963, 206. In *Dial. di Arch.* iii (1969) 118f. Boardman's statement about chronology is more in tune with the evidence as a whole. For examples found in the West, see *BSA* lxviii (1973) 191f., where there is a useful general discussion of the shape by O. T. P. K. Dickinson; *NSc.* xxvi (1972) 256 fig. 36 (p. 246).

additional argument for a high chronology is the extreme paucity of skyphoi with pendent concentric semicircles at the new city of Eretria, which was founded, it seems, in the first half of the eighth century.[87] But if we look to the East, especially the Syro-Phoenician coast and Cyprus, it is hard to explain away all the examples for which dates as low as *c.* 700, or even a little later, have been proposed. In view of these examples it has been suggested that the reasons for the rarity of skyphoi with pendent concentric semicircles in the West was not chronological. (The distribution map of such finds in the East might be thought to suggest that they were traded by Phoenicians.) ||

So some of our evidence seems to give us a lower date for the production of these skyphoi in the first half of the eighth century, while other indications suggest *c.* 700. A compromise solution may perhaps be found in the idea that the production of these skyphoi in the Euboean area ceased in the first half of the eighth century, but they were manufactured and traded elsewhere for almost another hundred years. If that is so, the presence of this sherd at Thasos is no great embarrassment for Bernard's chronology.

The date given by Bernard to the big bottom level, W, of sondage I, i.e. first half of the seventh century, depended on (*a*) his interpretation of the stratigraphy, (*b*) the analogies for the fragmentary apsidal house, and (*c*) the ceramic evidence. Of these only the last could offer any precision. Our examination of it confirms the correctness of his lower terminus; all the dating indications which can be discovered suggest that his material is in general terms earlier than the second half of the seventh century. On the other hand, the upper terminus seems to be rather less secure. We have found no decisive evidence to exclude the possibility that the material from level W goes back into the eighth century.

In the historical interpretation of this early material from Thasos the first and most fundamental question is: who were the people who lived in these houses and used this pottery? If we may be permitted to discern a chronological progression in the interpretation offered by the French Thasian experts, it seems that their first con-

[87] I am indebted to Professor J. N. Coldstream for information on this point. On the new Eretria, see the brief, clear, account by Paul Auberson, "Chalcis, Lefkandi, Erétrie au VIIIe siècle," *Cahiers du Centre Jean Bérard* II (Naples, 1975) 9-14. For skyphoi with pendent concentric semicircles at Eretria, see *AAA* iii (1970) 314ff.

clusion was that this settlement antedated the Parian colonization of the island. The material from the two sondages was briefly described by Salviat in his contribution to the *VIIIe Congrès international d'Archéologie Classique*, Paris, 1963.[88] He regarded it as evidence for the native Thracian culture which preceded the Parian colonization. Similar ideas can be seen in the *Chronique*, where the excavations were first briefly reported.[89]

The reasons for this interpretation were clearly stated. The finds from these two sondages were as a whole strikingly different from anything previously known from Greek Thasos. Several categories of pottery were either totally unknown or exceptionally rare in all the large quantity of material recovered from earlier excavations. In addition, the trading and cultural influences on the inhabitants, as revealed by the very varied pottery discovered in the two sondages, came from directions different from those previously known at Thasos. There is a most striking absence of the Cycladic material, which is the dominant ceramic influence in finds at Thasos from *c.* 650.[90] In view of this it is not at all surprising that the preliminary conclusion should have been that here for the first time there had been discovered evidence for the pre-Greek inhabitants of Thasos, whose presence was universally postulated from the literary evidence and other considerations.[91]

This simple and attractive interpretation was, however, replaced by more complex conclusions when the material was fully published by Bernard. His interpretation starts from his chronology. Since he dated his material to the first half of the seventh century, and since he regarded the date of the foundation of the Parian colony as certainly in the first quarter of the seventh century, it followed that his material did not antedate the Parian colonization,[92] but was contemporary with the beginnings of the Thasian polis. In this context he found the destructions by fire of levels W and Y suggestive. They

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[88] *Actes du VIIIe Congrès etc.* (Paris, 1965) 299-303; see especially 302f.

[89] *BCH* lxxxvi (1962) 936: "vestiges d'un habitat qui semble avoir précéde l'occupation parienne . . ."; cf. lxxxv (1961) 936: "poterie indigène contemporaine de la colonisation parienne ou même antérieure à celle-ci."

[90] See especially Salviat, *ibid.*, though the facts are more fully presented in Bernard, *passim.* On the Cycladic influences after *c.* 650, see also Boardman, *Greek Overseas*^{2}, 225.

[91] See Bernard, 143f., where these arguments are briefly listed together with useful bibliographical notes.

[92] For this and what follows, see his 'Conclusion,' 142-6.

reflect, he thought, the violent struggles of the early days of Thasos attested by the fragments of Archilochus and the legend that Heracles captured the island by force.

However, even though level W is thought by Bernard to show us the beginnings of the Greek colony, he considers that the material attests the prehellenic culture which the superior Greek civilization was very soon to supplant. Here he adduces in particular the pottery of local make with its strong Thraco-Macedonian characteristics. But this so-called prehellenic culture, he warns, is not *chronologically*[93] earlier than the Parian colonization. Since analogies suggest to him that the house is Greek, he thinks it better to attribute its destruction to a violent reaction on the part of the native Thracian inhabitants. How then does he explain the association of prehellenic native culture with the presumed Greek occupants of the house? By the suggestion that the recently installed Parian colonists were obliged to borrow the instruments of their material life from the local population.[94]

Bernard also offered explanations for the unusual directions of the trade of this early settlement as compared with later Thasos. The eastern influences he compared with evidence for contacts with the east and eastern tastes in later Thasos. As for the surprising absence of Cycladic material, perhaps the difficult circumstances of the new colony discouraged traders from the Cyclades from risking themselves in the new market.[95] In brief, then, Bernard's historical interpretation was that the settlement revealed by the sondages in Champ Héraclis and Champ Dimitriadis shows us the 'cité Thasienne à peine naissante' (p. 146), to be overtaken about 650 by 'cette croissance vigoureuse' (p. 77), the Greek, and notably Cycladic, character of which we have long known from many earlier discoveries.

Pouilloux offered a different interpretation again.[96] Although following Bernard's archaeological conclusions, and indeed quoting part of his historical interpretation,[97] he departs from that in seeing the settlement as native rather than Greek.[98] Nor does he agree with

[93] His italics, p. 144.
[94] Bernard, 145.
[95] Bernard, 146.
[96] *Fond. Hardt* x, *Archiloque* (Geneva, 1964) 1-27.
[97] *Op. cit.* 15.
[98] See his description of the Thracian inhabitants, 14f., and especially the remark (15) that these habitations were perhaps discovered by Telesicles when he came to found the new city.

Bernard's picture of struggles between these Thracians and the Parian colonists, for Pouilloux believes on the contrary that the relations between the Greeks and the natives were fundamentally good.[99] His notion is of one or even two generations of co-existence between the incoming Parians and the native inhabitants. First the 'missionaries' arrived at the end of the eighth century with Tellis, Archilochus' grandfather, who is supposed to have introduced the cult of Demeter to the island. Then came the first 'political' settlement *c.* 680 under Telesicles, Archilochus' father, and, finally, the strong Cycladic influences of *c.* 650 were perhaps brought by the influx of further Parian settlers, i.e. Archilochus and his companions.[100] These ideas are fundamentally the result of Pouilloux's interpretation of the literary evidence, which we shall consider in the next section; but it is not surprising that at least one of Pouilloux' auditors found his concept of Greek 'missionaries' without political support hard to credit in Greek colonization as we know it.[101]

When these interpretations were offered there existed virtually no archaeological evidence for the pre-Greek inhabitants of the island of Thasos. We now know of two prehistoric settlements, one in a cave near the village of Maries on the west coast, the other on a peak called Kastri in a mountainous region in the south part of the island, between Theologos and Potos.[102] Since the surface material in the cave dates from Neolithic to Early Bronze, the first site lies outside the context of our discussion. At Kastri the evidence from settlement and cemetery together attests occupation from Late Neolithic to Early Iron Age. It is particularly significant for the || history of the island that some of the pottery from the Late Bronze Age graves at Kastri is closely comparable with the Knobbed Ware of Troy VIIB2. For Troy VIIB2 has long been recognized as a Thracian settlement. This archaeological evidence therefore agrees with the literary sources about the pre-Greek population of Thasos.[103] In this connection the general character of the cemetery is also important. The graves are in groups, which the excavator understandably thought represented

[99] *Op. cit.* 15f.

[100] *Op. cit.* 22f., 16f.; see also *Et. Thas.* III 25f., 330.

[101] Kondoleon, in the *Discussion, Fond. Hardt* x. 36.

[102] For the position of the sites, see D. Lazaridis, *Thasos and its Peraia* (Athens, 1971) figs. 20, 23, 26, etc. The investigations at the two sites are published by H. Koukouli-Chrysanthaki, *in Arch. Eph.* (Chr.) 1970, 16-22, and *AAA* iii (1970) 215-22.

[103] *Arch. Eph.* (Chr.) 1970, 18f.

families. This is reminiscent of the great cemeteries at Vergina and Pateli.[104] Another discovery of great interest for our present subject is that in a burial of the Iron Age there was found a jug which the excavator convincingly attributed to G 2-3 Ware.[105] Combining the material from cemetery and settlement she reported that there is nothing at Kastri later than about the early seventh century.[106]

The excavator naturally compared her material from Kastri with that from the two sondages in the city of Thasos. Writing in 1970 she noted that the material from the two sondages had been attributed to the time of the first Parian colonists, and stated that it was still a problem whether or not there had been a Thracian settlement on the site of the city of Thasos.[107] In 1973, on the other hand, she confidently called the earliest material from these two sondages pre-colonial, though without giving her reasons for the change of mind.[108]

It is clear that the historical interpretation of this early archaeological material from Thasos depends very much on considerations of chronology. When Salviat was interpreting the material as native and pre-Greek, he was dating it to the end of the eighth century and beginning of the seventh.[109] Once the dating was lowered to the first half of the seventh century, ideas like the co-existence of Pouilloux, or Bernard's that the Greeks used the native material objects, or other unsatisfactory compromises, became unavoidable, so long as the date of the Parian colonization is taken to be *c.* 680. That date is based on literary evidence, which it is now necessary to reconsider.

Literary evidence

If it were not for the involvement of Archilochus the literary evidence for the foundation of Thasos would be slight.[110] We should

[104] See Hammond, *History of Macedonia* I, 328ff., 340ff.

[105] *Arch. Eph.* (Chr.) 18, 1970, 21f.

[106] *AAA* iii (1970) 220.

[107] *Op. cit.* 215f.

[108] *AAA* vi (1973) 240.

[109] *Actes du VIIIe Congrès d'Arch. Class.* 302. In the *Guide de Thasos* (preface by G. Daux, Paris, 1967) 2, a compromise is attempted: "vestiges d'habitations qui pourraient correspondre à une phase légèrement antérieure a l'installation des colons."

[110] Cf. Jacoby, 'The date of Archilochus', *CQ* xxxv (1941) 97-109 (hereinafter simply Jacoby), especially 102f. For the literary evidence, see E. Jacobs, *Thasiaca*

have, on the one hand, the bare statements of Thucydides and Strabo that Thasos was a Parian foundation,[111] and, on the other, the mythical stories of foundation by the eponym Thasus and of conquest by Heracles.[112] The foundation date given by Eusebius, 1425 BC,[113] belongs to the second category and can be simply explained (and dismissed). Thasus was stated by Herodotus to be a Phoenician, and so his story became linked with Cadmus, because all Phoenicians in early Greek history were attracted to the figure of Cadmus.[114] The preceding item in Eusebius concerns Cadmus and Thebes.

Turning to consider the literary sources which are directly or indirectly Archilochan, we may begin with the only other date(s) for the foundation of Thasos preserved in our literary tradition. Clement of Alexandria[115] tells us that Xanthus of Lydia dated it to about the eighteenth || Olympiad, but Dionysius (of Halicarnassus, almost certainly)[116] to about the fifteenth. These dates (708 or 720, to take the first year of each Olympiad mentioned) are at first sight attractively plausible, and they have been taken in the past by not negligible authorities as representing in general terms a good historical foundation date for Greek Thasos.[117] In addition the passage of Clement in which they are preserved has presented considerable difficulties of detailed interpretation, difficulties which are not fully disposed of in the discussions known to me.[118] A detailed examination is therefore required.

Clement's general aim in this part of the *Stromata* is to show that Hebrew culture was more ancient than pagan.[119] To do this he

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(Berlin, 1893) 6-12; *IG* XII. 8 pp. 75ff. (C. Fredrich, 1909); *RE* s.v. Thasos 1310-27 (F. Hiller von Gärtringen, 1934); J. Pouilloux, *Et. Thas.* III 22ff.

[111] Thuc. IV 104.4; Strabo, X, 487.

[112] Hdt. II 44. 3-4; VI 47. 1; Ps-Scymnus 659-64; Apoll. *Bibl.* II 5. 9. 3-5 and 13; *IG* XIV 1293A, 83-4.

[113] Ed. Schoene, II 34; Hieronymus, *Chron.* ed. P. Helm (*Die Griech. Christl. Schrifts. d. erst. Jahrh.*, Eusebius VII; Berlin, 1956) 48b.

[114] Cf. Jacobs, *op. cit.* 12; note the express connection of Thasus and Cadmus in Ps-Scymnus, *loc. cit.*

[115] *Stromata* I 131.7 (*Die Griech. Christl. Schrifts.* etc., Clem. Alex. II, Berlin, 1960, p. 81.)

[116] It is universally taken to come from Dionysius of Halicarnassus' Χρόνοι; see Jacoby, 99 n. 1 and *FGH* 251 fr. 3, with commentary *ad loc.*

[117] Jacoby, 102 n. 4; A. Blakeway, "The date of Archilochus," in *Greek Poetry and Life. Essays presented to Gilbert Murray* (Oxford, 1936) 51.

[118] Jacoby, 100 with nn. 1 and 2; 99 n. 2; 103 n. 2; Blakeway, *op. cit.* 50.

[119] This rather simple statement is enough for our present purposes. For the many complicated questions about the character and aims of the *Stromata*, see André

involves himself in questions of chronology, and the passage with which we are concerned is part of a discussion of the relative chronology of Greek poets/musicians. In particular, at this point he is arguing the relative position of Terpander. The general logical context of our passage begins with the statement "Indeed some make Terpander ancient."[120] I give a plain and literal translation from that point.

> Hellanicus at least says that he lived[121] in the time of Midas [*c.* 738 to 696, Eusebius]; but Phanias[122] puts Lesches of Lesbos before Terpander and makes Terpander more recent than Archilochus, and he says that Lesches competed with Arctinus and won the victory. But Xanthus the Lydian says that Thasos was founded about the eighteenth Olympiad (according to Dionysius it was about the fifteenth), so it is manifest that Archilochus was well-known after the twentieth Olympiad. At any rate there is the additional point that he mentions the destruction of Magnesia as a recent occurrence etc.[123]

The difficulty of understanding the logic of the argument that, if Thasos was founded in the eighteenth (or fifteenth) Olympiad, Archilochus was clearly well-known after the twentieth, led one early editor to pronounce the passage corrupt.[124] That is perhaps still a possibility, but, since it is clear (as Jacoby pointed out)[125] that Clement is abbreviating some fuller chronographical argument in his source, we should perhaps rather conclude that he has omitted some part of the argument as sufficiently obvious not to need stating. And that part of the argument must be, as Blakeway said,[126] that Archilochus was in some way connected with the colonization of Thasos. But this explanation of the general nature of the 'logical lacuna,'[127] though

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Méhat, *Étude sur les 'Stromates' de Clement d'Alexandrie* (*Patristica Sorbonensia* 7, Paris, 1966).

[120] ναὶ μὴν καὶ Τέρπανδρον ἀρχαΐζουσί τινες (I. 131. 6.).

[121] That this is the correct translation of γεγονέναι, and not 'was born', was shown in general by Rhode, *Rh. Mus.* xxxiii (1878) 162f.

[122] I.e. Phainias of Eresos; see Jacoby, 100 n. 1.

[123] Ἑλλάνικος γοῦν τοῦτον ἱστορεῖ κατὰ Μίδαν γεγονέναι, Φανίας δὲ πρὸ Τερπάνδρου τιθεὶς Λέσχην τὸν Λέσβιον Ἀρχιλόχου νεώτερον φέρει τὸν Τέρπανδρον, διημιλλῆσθαι δὲ τὸν Λέσχην Ἀρκτίνῳ καὶ νενικηκέναι· Ξάνθος δὲ ὁ Λυδὸς περὶ τὴν ὀκτωκαιδεκάτην ὀλυμπιάδα (ὡς δὲ Διονύσιος, περὶ τὴν πεντεκαιδεκάτην) Θάσον ἐκτίσθαι, ὡς εἶναι συμφανὲς τὸν Ἀρχίλοχον μετὰ τὴν εἰκοστὴν ἤδη γνωρίζεσθαι ὀλυμπιάδα, μέμνηται γοῦν καὶ τῆς Μαγνήτων ἀπωλείας προσφάτως γεγενημένης κτλ.

[124] John Potter; see *Clementis Alexandri Opera* (Oxford, 1715) p. 398 n. 2.

[125] 99 n. 2.

[126] *Op. cit.* 50f.

[127] *Ibid.*

incontrovertible, is not the complete answer, since it still fails to tell us why Clement thought that Archilochus' connection with the colonization of Thasos in the eighteenth (or fifteenth) Olympiad should clearly prove that he was well-known after the twentieth. Hence the further suggestion of Jacoby: "If there are twenty years between the two dates, he knew from Kritias that Archilochus emigrated to Thasos as a young man."[128] But that would only explain some such train of thought as "he took part in the colonization of Thasos when he was young, therefore the time when he was well-known was later than the date of the foundation of Thasos." The explanation of the twentieth Olympiad is still required. ||

Surely Clement's very truncated argument here must refer to Terpander, whose dates he is seeking to fix in relation to Archilochus'. The dates that he has given for Terpander's activity (from Hellanicus) are the reign of Midas. Midas' reign, according to Eusebius, lasted from 738-696, or down to the first year of the twenty-first Olympiad. We know that Clement was not using the same chronographical source as Eusebius.[129] I suggest, therefore, that for Clement the twentieth Olympiad represented the lower date of Midas' reign. We may thus re-word his chronological argument as follows. We have a *terminus ante quem* for Terpander's activity in the end of the reign of Midas, i.e. twentieth Olympiad. For Archilochus', on the other hand, we have a *terminus post quem* in the foundation of Thasos, i.e. eighteenth Olympiad (notice how he puts first the lower date of the two he has found for Thasos' foundation). Therefore it is obvious that Archilochus' activity must be later than Terpander's, since the *terminus post quem* for Archilochus is only just earlier than the *terminus ante quem* for Terpander.

If this interpretation is on the right lines, there is no reason to suppose that Clement was thinking, when he was writing this passage, of any participation of Archilochus in the foundation of Thasos. For his argument all that is necessary is the knowledge that Parian Thasos existed in Archilochus' lifetime, a fact obvious from many of the poems. All such notions as that he knew that Archilochus took part in the foundation of Thasos when he was young,[130] or that he knew Archilochus cannot have been, a child when he wrote the

[128] 100 n. 3.
[129] See W. Christ, *Abh. Bayr. Akad.* xxi. 3 (1901) 509.
[130] Jacoby, 100 n. 3.

Thasian poems,[131] or even "that he took it as a well-established fact . . . that Archilochus had played some part in the foundation of Thasos"[132] (not to mention such far-fetched ideas as that he took the foundation of Thasos as the *terminus post quem* for the birth of Archilochus)[133] become entirely otiose. It is very probable that Clement knew the tradition that Archilochus had been a settler at Thasos, but for the purposes of his argument that was unnecessary. The only connection that he needed to make between Archilochus and Thasos was that the colony was in existence by the time of the poetic activity of Archilochus. It is therefore quite erroneous to draw from this passage any conclusions about Clement's ideas of Archilochus' role in the colonization of Thasos.[134]

It is also clear from Clement's (or his source's) chronological argument that he regarded the date(s) he had for the foundation of Thasos as independent of Archilochus; otherwise they would be of no use in dating the activity of the poet. Unfortunately for Clement, however, and for us, it is virtually certain that these dates, far from being independent of Archilochus, are the results of two synchronisms, (*a*) Archilochus-Gyges and (*b*) Archilochus-Thasos. This was suggested long ago by von Gutschmid,[135] and the argument was spelt out in detail by Rohde.[136] The two dates given by Xanthus of Lydia and Dionysius for the foundation of Thasos are both the same as dates given in other sources for the beginning of Gyges' reign.[137] Such a coincidence, when we have two dates in question, is hard to explain away. It is not surprising that von Gutschmid's and Rohde's theory was accepted by Jacoby,[138] and has, to my knowledge, never been seriously questioned.[139]

So far from having independent dates for Archilochus from the foundation date of Thasos, Clement (or his source, no doubt) was inadvertently arguing in a complete circle. There was no traditional

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[131] Jacoby, 103 n. 2.

[132] Blakeway, *op. cit.* 51.

[133] Jacoby, *ibid.*

[134] Here Jacoby followed the false lead of Blakeway, even though he disagreed in detail.

[135] A. von Gutschmid, *Kleine Schriften* (Leipsig, 1892) III 474, but first published in 1885. See, however, Rhode's acknowledgement (next note).

[136] *Rh. Mus.* xxxiii (1878) 194f.

[137] Most clearly Pliny *N.H.* XXXV, 23.

[138] *Apollodors Chronik* (*Phil. Unters.* xvi, Berlin, 1902) 143f.

[139] Cf. Pouilloux. *Et. Thas.* III 23 n. 7.

date for the Parian colonization of Thasos.[140] Calculation via Archilochus and Gyges was the only substitute available. As Jacoby has clearly shown, that method was bound to lead || most of the chronographers to too high a date, because their Mermnad chronology was too high[141] (leaving on one side the question what date *within* Gyges' reign they picked on). Since we now possess good dates for Gyges (i.e. *c.* 680-652),[142] we can at least define the target area correctly, so long as we accept the two synchronisms Archilochus-Gyges and Archilochus-Thasos. Since Jacoby's magisterial paper, no one is likely to doubt the former. Archilochus' poem about Gyges will have been composed either during Gyges' lifetime, or, at the latest, not long after his death.[143] Our view of the synchronism Archilochus-Thasos depends on the way we interpret Archilochus' connection with Thasos. We know from Critias that he went there as a settler,[144] but there remains the further question whether he took part in the original expedition or represents a subsequent reinforcement. If the latter, the synchronism Archilochus-foundation of Thasos is ill-defined.

In modern times this question is normally answered confidently and definitely by reference to the oracle about the foundation of Thasos preserved in Stephanus Byzantinus (s.v. Θάσος) and a passage of Oenomaeus of Gadara reproduced by Eusebius (*Praep. Evang.* vi. 256b). This oracle is taken to show the Archilochus' father Telesicles was the *oikistes* of Thasos, and the foundation is, therefore, placed about a generation before the *floruit* of Archilochus, when the poet himself took part in a reinforcement of the colony.

This idea was very plainly and confidently expressed by Myres:[145] "The date of the colony is certain, in the generation before Archilochus,"

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140 Cf. Jacoby, 102 n. 4.

141 Jacoby, 101.

142 H. Kaletsch, 'Zur Lydischen Chronologie'. *Historia* VII (1958) 1-47. The date of Gyges' death is almost certain (Kaletsch, 29f.; cf. Jacoby, 99) but the beginning of his reign is only to be achieved by calculation. Jacoby was prepared to use the duration of his reign given in Greek sources and the date of his death to give the dates about 687-652 (*ibid.*). Kaletsch rejects this procedure as unhistorical, and more cautiously chooses *c.* 680. after a full discussion of the various indications (30-4).

143 Jacoby, 109; cf. 101 n. 7 and 107.

144 Frg. 44 = Aelian V.H. x. 13, printed in M. Treu, *Archilochus* (Munich, 1959) 120, and *Archilochus*, ed. I. Tarditi (Rome, 1968) T. 46. The sense of the words . . . οὐκ ἂν ἐπυθόμεθα . . . οὔθ' ὅτι καταλιπὼν Πάρον διὰ πενίαν καὶ ἀπορίαν ἦλθεν εἰς Θάσον οὔθ' ὅτι ἐλθὼν τοῖς ἐνταῦθα ἐχθρὸς ἐγένετο . . . seems clear. There are many other texts which bear on this question; see below pp. 84ff.

145 *CAH* III 654 (1929).

and, even if more recent writers have expressed themselves more cautiously, a generation before Archilochus is still the ruling principle in their discussions of the date.[146] Thus *c.* 680 is now the favoured date for those who hazard a close approximation,[147] a generation before Archilochus, whose *floruit* is placed at *c.* 650. It is clear that this chronological argument depends not only on the belief that Telesicles, Archilochus' father, was the founder of Parian Thasos, but also on the assumption that the act of foundation took place a generation earlier than the son's *floruit.* It is not surprising that there have been objections to the second of these assumptions, which is obviously not beyond question. However, even those who have not accepted that the date should be a generation before Archilochus have not doubted that Telesicles was the *oikistes* of Greek Thasos.[148]

It is time to examine the oracle which has such an important place[149] among the literary sources for the foundation of Thasos. Although the oracle appears in the same words in both Stephanus of Byzantium and Oenomaeus of Gadara, the latter provides comments upon it which are enlightening. We may therefore approach it via Oenomaeus (that is to say in Eusebius, *Praep. Evang.* vi. 256b).[150] ||

There are no textual difficulties, and all the editors of Eusebius known to me print the passage similarly (as Parke and Wormell also print the oracle), i.e.

> ἄγγειλον Παρίοις, Τελεσίκλεες, ὥς σε κελεύω
> νήσῳ ἐν Ἠερίῃ κτίζειν εὐδείελον ἄστυ.

Mullach translates,

> I Parios mone, Telesicles, numine nostro,
> insula in Aeria sublimem sedibus urbem
> condere te iussum.

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[146] As, for example, Pouilloux; see above p. 71: F. Salviat and N. Weill, *BCH* lxxxiv (1960) 383.

[147] E.g. L. Ghali-Kahil, *Et. Thas.* VII 139; J. Boardman, *Greeks Overseas*², 225; J. Bérard, *L'Expansion et la Colonisation Grecques* (Paris, 1960) 92; *Guide de Thasos* 2.

[148] A. R. Burn, *JHS* lv (1935) 122. Jacoby, in a rather confused note, 102 n. 4, prefers two generations, but earlier accepts that the founder is Telesicles, according to the oracle.

[149] Cf. Pouilloux, *Et. Thas.* III 24: "L'importance de ce texte a été dès longtemps reconnue."

[150] See *Die Griech. Christl. Schrifts.* etc. Eusebius VIII.1 (Berlin, 1954) p. 314; Eusebius. *Praep. Evang.* ed. F. H. Gifford (Oxford, 1903) i. p. 328f.; for translation,

Gifford,

Tell thou the Parians, Telesicles,
I bid thee found in the Aërian isle
A city fair to view.

Parke and Wormell (I. p. 66), Announce to the Parians, Telesicles, that I bid you found a conspicuous city in the island of Eeria.

Pouilloux' interpretation,[151] "Télésiklès fut l'oikiste de la cité nouvelle; c'est a lui que selon l'oracle . . . le dieu de Delphes donna l'ordre de partir fonder la ville qu'on voit de loin", presupposes the same translation.

This way of translating the oracle is standard throughout the editions of Eusebius that I have checked. Notice that in this rendering ὥς in the phrase ὥς σε κελεύω is taken to mean *that*, a construction which is, in itself, perfectly legitimate after ἀγγέλλω.[152] There is, however, another way of taking it; ὥς could mean *as*, and the phrase ὥς σε κελεύω would then be parenthetic. In that case ἄγγειλον would be followed by the plain infinitive κτίζειν, also in itself a perfectly legitimate construction after ἀγγέλλω.[153] Such a translation of the oracle is to be found, for example, in A. Berkelius' translation in his edition of Stephanus Byzantinus,[154] viz.,

Annuncia Pariis, Telesicles, quemadmodum jubeo,
In insula Eeria ut condant praeclaram urbem.

John Potter cited the oracle from Steph. Byz. in his note to the passage Clem. Alex. *Stromata* I. 131. 7, and translates as follows,

Dic Pariis, Telesicles, ut te jubeo,
Insula in Aeria urbem claram ut condant.[155]

It is notable that the editors of Stephanus Byzantinus have regularly put a comma after κελεύω, thus indicating that they took the phrase parenthetically.[156]

III. 1 p. 276; for notes. IV p. 207f.; *Frag. Phil. Graec.*, ed. F. G. A. Mullach (Paris, 1867) II frg. 14 (pp. 380f.). For the oracle, cf. also H. W. Parke and D. E. W. Wormell, *The Delphic Oracle* (Oxford, 1956) II no. 230.

[151] *Et. Thas.* III 24.

[152] See Liddell and Scott, s.v., I. 3; e.g. Eur. *IT* 704.

[153] Liddell and Scott, s.v., I. 1; e.g. *Od.* XVI. 350.

[154] *Stephani Byzantini Gentilicia* (Leyden, 1694) s.v. Thasos.

[155] See n. 124 above.

[156] E.g. the edition of L. Holsten and others, Leipsig, 1825, and *Steph. Byz.* Ἐθνικῶν *quae supersunt*, ed. A. Westermann (Leipzig, 1839), s.v. Θάσσος.

In seeking the correct choice between these alternative interpretations we may begin by examining Oenomaeus' comments on the oracle. Ἀγγελῶ νὴ Δία (φήσει τάχα που τις τετυφωμένος ἢ σὲ ἐλέγχων) κἂν μὴ κελεύσῃς· πέπρωται γάρ. καὶ ἔστι Θάσος μὲν ἡ Ἠερία νῆσος· ἥξουσι δ' ἐπ' αὐτὴν Πάριοι, Ἀρχιλόχου τοῦ ἐμοῦ υἱοῦ φράσαντος ὅτι ἡ νῆσος αὕτη πρὶν Ἠερία ἐκαλεῖτο. σὺ οὖν, δεινὸς γὰρ ἐπεξελθεῖν, οὐκ ἀνέξῃ, οἶμαι, αὐτοῦ οὕτως ὄντος ἀχαρίστου καὶ θρασέος, ὅς, εἰ μὴ σὺ μηνῦσαι αὐτῷ ἐβουλήθης, οὐκ ἄν ποτε ἤγγειλεν οὐδ' ἂν Ἀρχίλοχος ὁ υἱὸς αὐτοῦ Παρίους ἐξενάγησεν οὐδ' ἂν οἱ Πάριοι Θάσον ᾤκησαν. ||

This passage, which Jacoby found 'rather verbose',[157] is part of Oenomaeus' general argument against oracles, that, if events are ordained by fate, it does not matter whether they are ordered by Apollo's oracle or not.[158] We may render the passage as follows.

> I shall certainly bear the message (someone will perhaps say with insane arrogance or in order to refute you), even if you do not order me. For it is fated. And Thasos is the Eërian isle; the Parians will come to it when my son Archilochus has told them that island was formerly called Eëria. Then you, I imagine, because you are terrible in vengeance, will not put up with such ingratitude and audacity; for if you had not been willing to inform him, he would never have borne the message, nor would his son Archilochus have shown the Parians the way,[159] nor would the Parians have settled Thasos.

What are the events which Oenomaeus makes Apollo claim would not have happened if he had not ordered them? (1) Telesicles would not have announced the order of the god; (2) Archilochus would not have shown the way; (3) the Parians would not have founded Thasos. What action does Telesicles argue that he would have performed, whether Apollo ordered it or not? Announcing the oracle to the Parians. Note particularly the combination ἀγγελῶ νὴ Δία . . . κἂν μὴ κελεύσῃς, and the later οὐκ ἄν ποτε ἤγγειλεν.

If we accept the current modern interpretation of the oracle this concentration on the announcing as Telesicles' action must seem strange. We should surely expect Oenomaeus to make Telesicles claim that he would have founded Thasos, whether or not Apollo ordered it, and to make Apollo reply that without his oracle Telesicles would not have founded the colony. For Oenomaeus it is the Parians

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157 Jacoby, 103.

158 Cf. Parke and Wormell, *op. cit.* II p. xi.

159 For this translation, see below pp. 80f.

in general who found the colony, while Telesicles' role is to announce the oracle.

There is nothing surprising about Oenomaeus' comments, however, if the correct way to take the oracle is the second alternative indicated above. If he took ὥς σε κελεύω parenthetically, Oenomaeus had no reason to understand the oracle as stating that Telesicles was *oikistes* of Thasos. That he would be likely to take ὥς σε κελεῦω parenthetically is suggested by the use of this and similar phrases in Homer, whose language established the modes of epic diction.

There is a reasonably close parallel in *Odyssey* X. 516f.: ἔνθα δ' ἔπειθ', ἥρως, χριμφθεὶς πέλας ὥς σε κελεύω, βόθρον ὀρύξαι κτλ., where we have the phrase used parenthetically, when it is strictly unnecessary. At *Hymn to Aphrodite* 283, (ἢν δέ τις εἴρηται σε...) τῷ δὲ σὺ μυθεῖσθαι μεμνημένος ὥς σε κελεύω, the usage is, in principle, similar. In addition the phrases like ὡς σὺ κελεύεις (e.g. *Il.* viii. 35, *Od.* iv. 485, etc.) and ὥς με κελεύεις (e.g. *Od.* xi. 507, etc.) are so common[160] that I think we can say that the Greeks will have naturally taken ὥς with κελεύω in verse as meaning *as*, and to take it as meaning *that* would be most unnatural.[161]

Another argument for taking ὥς σὺ κελεύω as a parenthesis was put to me in conversation by Professor F. H. Sandbach. The nub of the oracle is clearly in the second line, νήσῳ ἐν Ἠερίῃ κτίζειν κτλ. But, if we take ὥς as meaning *that*, the main message of Apollo becomes 'that I am ordering you', which, at the least, takes away from the force of what follows.

We may conclude, therefore, that the second way of understanding the Greek of the oracle is certainly the correct one. It follows that Telesicles was not ordered by Apollo to found the colony at Thasos, and that Oenomaeus did not think of him as its *oikistes*. This conclusion clarifies a fact which is certainly surprising, if we take Telesicles to be *oikistes* of Thasos, viz. that no ancient authority says so. Although an *argumentum e silentio*, this one is not without weight, || when one considers the interest of the ancients in *ktiseis* and in Archilochus. Since the oracle was presumably widely available,[162] we

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[160] See G. L. Prendergast, *Concordance... Iliad*, and Henry Dunbar, *Concordance... Odyssey*, s.v. κελεύεις.

[161] As far as the concordances allow, I have checked all occurrences of ὡς with κελεύω. It always means *as*.

[162] Cf. Parke and Wormell, *op. cit.* II, Prolegomena, especially xvi-xxi.

may at least conclude that it was not only Oenomaeus who saw in it nothing which makes Telesicles *oikistes* of Thasos.

It may not be too hazardous to apply the *argumentum e silentio* further. Since Oenomaeus will have been acquainted with the available material about Archilochus and Thasos—that is shown by his knowledge about Eëria—we may perhaps also conclude that there was no other evidence, apart from the oracle, which could be taken to show that Telesicles was an *oikistes*, for, if there had been such evidence, Oenomaeus would have known of it, and his knowledge would have been apparent in his comments.

It is necessary to venture into these speculative regions because a modern investigator might well argue that, however one interprets the exact wording of the oracle, the mere fact that the oracle about the foundation of Thasos was given to Telesicles shows that he was the *oikistes.*[163] Thus Parke and Wormell write "ENQUIRY. Not precisely stated. For a colony?;" and in their general discussion they suggest that "One can take it that . . . the Parians had sent Telesicles to ask for Apollo's sanction etc."[164] The argument could therefore run, even though the ancients apparently missed the important implication, if there was a genuine foundation oracle for Thasos addressed to Telesicles, prima facie he is likely to have been the *oikistes.* Furthermore, even if he was not the *oikistes,* if there was a genuine foundation oracle given to Archilochus' father, the chronological conclusions drawn from the oracle would be unchanged. So we need to consider if the oracle was genuine or a later invention.

There are two ways to approach this question, by investigating the general context of the oracle, and by examining the oracle itself. In our tradition there are two oracles given to Telesicles, the foundation oracle of Thasos and the oracle prophesying his son's greatness (Parke and Wormell, II, nos. 230 and 231). In addition we have the oracle given to Archilochus, ordering him to settle in Thasos (no. 232), and the hostile responses given to his killer, when he arrived at Delphi (nos. 4 and 5). In the first edition of his book Parke took the foundation oracle for genuine,[165] an idea which was

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[163] Cf. A. S. Pease, 'The Delphic Oracle and Colonisation', *CP* xii (1917) 1-20 especially 6: The enquirers are sometimes groups—cities, bands of exiles, families—and often individuals, usually the οἰκιστής.

[164] *Op. cit.* II no. 230 and I 66.

[165] *A History of the Delphic Oracle* (Oxford, 1939) 68.

enthusiastically espoused and developed by Pouilloux.[166] In the second edition, while the same words appear in volume I (p. 66), Parke and Wormell clearly regarded the oracle as inauthentic when they compiled volume II. For the oracle about the foundation of Thasos (no. 230) is there placed in their Fifth Period, i.e. between the beginning of the Peloponnesian War and the destruction of the temple in 373 BC.[167]

Their reasons for placing the oracle in this period, and so pronouncing it inauthentic, were, it seems, "the rather subjective grounds . . . of general style and character," if we may judge by the reasons that Parke gave for placing the neighbouring oracle given to Telesicles (no. 231, about his son's future greatness) in the same period.[168] Shortly after their book was written new material produced by epigraphical discoveries on Paros[169] put the argument on a more solid basis. Here, for the first time, the full context of the oracle which foretold to Telesicles his son's greatness was given. It is part of the story which begins with the charming account of the Muses' || gift of the lyre to Archilochus. After that Telesicles was sent to Delphi as θεοπρόπον χρησάμενον ὑπὲρ τῆς πόλεως. When he entered the oracle the god gave him the famous response ἀθάνατός σοι παῖς καὶ ἀοίδιμος κτλ.[170] It is not surprising that, when he came to discuss the material anew in the light of this document, Parke considered that his and Wormell's dating of that oracle was confirmed.[171] As a result Parke reiterated the view (which was implied by the placing of the relevant oracles in his and Wormell's book) that of the four oracles relating to Archilochus only the responses given to his killer are authentic. He concluded that "Probably by the end of the fifth century at latest a traditional account of Archilochus' career, made up

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166 *Et. Thas.* III 24f. However, he wrote more cautiously in *Fond. Hardt* x. 9: 'oracles—vrais ou apocryphes.'

167 There is a similar contradiction between the discussion about Archilochus and Delphi, I. 396f., and two of the oracles in question, II. nos. 231, 232. These and similar contradictions not surprisingly drew the criticism of H. Berve, *Gnomon* xxx (1958) 422f.

168 *CQ* n.s. viii (1958) 93.

169 N. M. Kondoleon, *Arch. Eph.* 1952, 32-95; 'Archilochos und Paros'. *Fond. Hardt* x. 39-54. There is a very good short account of the inscriptions in *Bull. Epig.* 1955, 178. See also *SEG* XV 517, where the texts are readily accessible. Also M. Treu, *Archilochos*, 152-4; *Archilochus*, ed. I. Tarditi, T4.

170 Col. II. 22-55 (for the visit to Delphi and the oracle, 43-52).

171 *CQ* n.s. viii (1958) 93.

of episodes linked with a succession of Delphic responses had been composed."[172]

It is interesting that similar, though more radical and fundamental, conclusions were reached *independently* by Tarditi in his subtle and perceptive discussion of the same new epigraphical material from Paros.[173] He observed that the story of Archilochus and the Muses is similar to the story about Hesiod,[174] and that both Hesiod and Archilochus are attested by Plutarch to have been honoured at Delphi for the sake of the Muses.[175] So he argued that Archilochus was absorbed into the ambit of Delphi, as a poet whose greatness was foretold by both Apollo and the Muses.[176] All the oracles relating to Archilochus are seen by Tarditi as concocted for a biography of Archilochus made at Delphi for this purpose between *c.* 477 and about the middle of the fourth century.[177] He considers that both the biography and the oracles were largely created out of the material in Archilochus' poems.[178] The responses given to Archilochus' killer, which Parke thought authentic, are shown to be concocted like the others, because there is a play on words which refers to Archilochus fr. 88A (Diehl).[179]

Breitenstein's criticism of some of Tarditi's ideas seem to me unconvincing, but he draws attention to an interesting point of detail in the oracle foretelling Archilochus' future greatness (Parke and Wormell, no. 231).[180] In early instances of its use (as by Homer and Stesichorus) the word ἀοίδιμος has a passive meaning, famed in song. From Pindar onwards it has an active sense, famous as a singer. In this oracle the meaning is rather the latter. Breitenstein sees this as an

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[172] *Op. cit.* 94. Cf. Parke and Wormell, *op. cit.*, II. nos. 4, 5 (responses to Calondas the Crow of Naxos, killer of Archilochus); 230 (foundation oracle of Thasos); 231 (response to Telesicles foretelling his son's greatness); and 232 (order to Archilochus to settle in Thasos).

[173] G. Tarditi, "La nuova epigrafe Archilochea e la tradizione biografica del poeta," *PP* xi (1956) 122-39. It is clear that Parke's paper, which appeared later, was written without knowledge of Tarditi's work.

[174] *Op. cit.* 130.

[175] Plut. *Numa* iv. 6; cf. Tarditi, 131.

[176] *Ibid.*

[177] Tarditi, 133f. On the dating, see 137f. It must be after Pindar's second Pythian (52ff.), which can be taken to show that Archilochus was still not in favour at Delphi, and before the first attestation of the story of his killer's reception at Delphi (Heracl. Pont. 8, of Aristotelian derivation).

[178] Tarditi, 133f.

[179] *Ibid.*

[180] T. Breitenstein, *Hésiode et Archiloque* (Odense, 1971), especially 9ff.

example of Delphi's notorious ambiguity, but what real ambiguity is there? And what would its purpose be? The way in which ἀοίδιμος is used in the oracle seems rather to show that the oracle is not early, and so to be another sign—albeit a small and uncertain one—that this oracle is not authentic.

So the foundation oracle of Thasos is to be placed in the wider context of an Archilochan biography, and, like the other oracles from this biography, pronounced inauthentic.

Even without these persuasive general arguments from context, if we examine the oracle itself we find it open to question. It belongs to the enigmatic class of colonization oracles, which Pease found good reasons for regarding with suspicion.[181] In this instance the recipients have to discover the meaning of νήσῳ ἐν Ἠερίῃ. Now it has long been recognized that Oenomaeus' words Ἀρχιλόχου τοῦ ἐμοῦ υἱοῦ φράσαντος ὅτι ἡ νῆσος αὕτη πρὶν Ἠερία ἐκαλεῖτο show that there was || a poem of Archilochus which stated that Thasos was previously called Eëria. Blakeway stated "Ἠερία *is in fact a fragment of Archilochus*,"[182] and he has been widely followed.[183] In view of this it is difficult not to conclude that the particular riddle chosen by the god reveals that his utterance was concocted on the basis of a poem of Archilochus. If that is right the form of the oracle is easily intelligible. The *oikistes* of Parian Thasos was not known; all that was known was the fact of the Parian colonization. So the oracle's command had to be directed to 'the Parians' in general, and the role of Telesicles is confined to announcing the oracle. Why Telesicles at all? Because, as Archilochus' father; Telesicles has an important role in the story of Archilochus and Delphi; he becomes the obvious Parian to be addressed by the god.[184]

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[181] *CP* xii (1917) 13.

[182] *Op. cit.* 50 (his italics).

[183] Jacoby, 103; F. Lasserre and A. Bonnard (Budé edition, Paris, 1958) frg. 263; *Archilochus*, ed. Tarditi, 246.

[184] Cf. *SEG* XV 517. A II 43-5. Even if we do not go as far as Tarditi (*PP* xi 131), who regards Telesicles as created for the purpose of his role in the Delphian biography of Archilochus. He may be right, since neither the epitaph of Archilochus, Ἀρχίλοχος Πάριος Τελεσικλέος ἐνθάδε κεῖται T¯ Δόκιμος μνημήιον ὁ Νεοκρέωντος τόδ' ἔθηκεν (*Ergon* 1961, 184f.; *BCH* lxxxv (1961) 846f.; *Bull. Epig.* 1962, 261; N. M. Kondoleon, *Fond. Hardt* x. 44-6. It was inscribed in the fourth century), nor the occurrence of the name Telesicles at Thasos (Pouilloux, *Et. Thas.* III. Catalogue I Col. 4.47 (opp. p. 262); *IG* XII, 8. 278, 17), nor even Critias' knowledge of the name of Archilochus' mother (frg. 44 = Aelian V. H. x 13) necessarily refute him. But perhaps the last does suggest that the name of Archilochus' father was preserved.

This discussion leads to the clear conclusion that Telesicles was neither regarded by antiquity as the *oikistes* of Thasos, nor was he in fact. Another candidate for the role has been Archilochus himself. The phrase of Oenomaeus οὐδ' ἂν Ἀρχίλοχος ὁ υἱὸς αὐτοῦ Παρίους ἐξενάγησεν has been taken to mean that Archilochus was the leader of the Parian colonists. Gaisford translated "nec Archilochus ejusdem filius Pariorum coloniam eo deduxisset."[185] and Gifford "nor would his son Archilochus have led the colony of the Parians."[186] He explained his translation as follows; "The verb [i.e. ἐξενάγησεν] means to act as ξεναγός (Thuc. ii. 75) or leader of foreign allies. The Parians would be ξένοι in Thasos."[187] The only attraction of such a rendering seems to be that Archilochus called himself a mercenary soldier,[188] but the objections are overwhelming. A ξεναγός, the captain of mercenaries, is quite different from an οἰκιστής, and the Parian colonists at least are quite different from mercenaries. To call them ξένοι in relation to the supposed native inhabitants would be most abnormal. We may therefore reject the translation of ἐξενάγησεν in the sense of military leadership.

The other meaning of ξεναγέω is to show a stranger or guest the way. Hence Blakeway wrote "ἐξενάγησεν means no more than acted as a guide."[189] This interpretation received rough handling from Jacoby,[190] and it is true that Blakeway did not argue the matter adequately, but he was probably on the right lines. In this sense the fundamental meaning seems to be that a native of a place guides a stranger round it. E.g. Lucian *Dial. Mort.* 5 (18) 1 (Loeb, VII, p. 20), ποῦ δαὶ οἱ καλοί εἰσιν ἢ αἱ καλαί, Ἑρμῆ; ξενάγησόν με νέηλυν ὄντα. (The question is put by Menippus to Hermes, who is the expert.) In the *Phaedrus* of Plato Socrates is made to end his speech praising the place to which Phaedrus has led them with the words, ὥστε ἄριστά σοι ἐξενάγηται, ὦ φίλε Φαῖδρε (230c). While Lucian, who uses the word quite frequently,[191] seems to keep to this meaning, a weakened, more general, sense of 'to guide' is certainly found later on.

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[185] Eusebius, *Praep. Evang.*, ed. T. Gaisford (Oxford, 1843) II p. 44.

[186] Eusebius, *Praep. Evang.*, ed. F. H. Gifford, III, 1 p. 276.

[187] IV p. 208.

[188] Frg. 40 D (= 38 in Tarditi's edition); cf. 2 D (= 2 in Tarditi's edition).

[189] *Op. cit.* 50.

[190] 102 n. 4: "Blakeway's compromise ... seems to me neither helpful nor probable."

[191] E.g. *Charon* (*Contemplates*) 1 (Loeb II p. 396; *Pisc.* (*Reviviscentes*) 12 (Loeb III p. 18); *Scytha* (*Hospes*) 4 (Loeb VI p. 246).

E.g. Themistius *Or.* ix. 123b, πρὸς τὰς Μούσας αὐτὸν ξεναγεῖ. Since the notion of Archilochus as someone familiar with Thasos showing the Parian strangers the way round is no less inappropriate than his being their mercenary leader, it is better to conclude that Oenomaeus used the word in || this general or weakened sense, to mean 'to show the way', referring quite precisely to the fact that Archilochus had interpreted νήσῳ ἐν Ἠερίῃ for the Parians and thus *shown them the way*. We thus obtain internal coherence and logic in Oenomaeus' argument and an intelligible sense for ἐξενάγησεν.

So we need not be led astray by any notion that Archilochus might have fulfilled the role of *oikistes* of Thasos.[192] We have now dismissed the father and the son. What of the grandfather (or great-grandfather)?

In his long and detailed description of the famous paintings by the Thasian Polygnotus in the Lesche of the Cnidians at Delphi, Pausanias tells us of two persons in Charon's boat, Tellis and Kleoboia.[193] He was told that the latter was the first to bring the rites of Demeter to Thasos from Paros. As for Tellis, all that he learnt was that the poet Archilochus was his descendant (ἀπόγονος . . . τρίτος Τέλλιδος).

The interpretation of this tantalizing information is complicated by an uncertainty in the text. The words Τέλλις μὲν ἡλικίαν ἐφήβου γεγονὼς φαίνεται have been suspected because (*a*) in the important MS. Leiden A the reading is Τέλλις μὲν ἡλικίαν γεγονὼς φαίης ἄν, and (*b*) it makes a poor contrast with the subsequent Κλεόβοια δὲ ἔτι παρθένος. So Wilamowitz proposed to read ἡλικίαν γέρων ὡς φαίης ἄν.[194] This bold solution has not been taken up by editors, but we should at least be wary of placing much weight on the exact words of the text as commonly printed.

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[192] Lines 17-21 of *P. Oxy.* 2310, which is attributed to Archilochus (54 in Tarditi's edition), have been interpreted as referring to Thasos and to Archilochus' own prospects of becoming ruler there; see F. Lasserre, *Mus. Helv.* xiii (1956) 233; cf. H. D. Rankin, *Ant. Class.* xliv (1975) 608f. Since we do not know who spoke those fragmentary lines, nor to whom they refer, nor to what city, no historical conclusions can be based on them. For the variety of possible interpretations, see Tarditi's apparatus, and, e.g. W. Peek, *Phil.* xcix (1955) 193ff.; Steffen, *Proc. IX Int. Cong. Pap., Oslo 1958* (1961) 18ff.; D. Page, *Proc. Camb. Phil. Soc.* vii (1961) 68f.

[193] X. 25-31, especially 28. 3. For a full discussion of the paintings, see Martin Robertson, *History of Greek Art* (London, 1975) I, 266ff.

[194] See *RE* s.v. Tellis, 1; J. G. Frazer, *Pausanias' Description of Greece* (London, 1898) critical note *ad loc.* (I p. 611).

There are no historical or chronological connections to help us place Kleoboia, so attention has naturally been concentrated upon Tellis.[195] Hiller von Gaertringen's idea that Tellis should be identified with Telesicles[196] still has at least one supporter in modern times,[197] but it becomes less and less attractive as the evidence accumulates which shows that Archilochus' father was known as Telesicles from, at the latest, the fourth century BC.[198] If the fifth-century painter had meant to depict Archilochus' father, he would surely have named him Telesicles. *Ergo* Tellis is what he meant to write, and Tellis is someone different. Both names are attested among historical people at Thasos.[199]

The next question is the number of generations between Tellis and Archilochus according to Pausanias' words, ἀπόγονος . . . τρίτος Τέλλιδος. Although generally taken to mean that Tellis was Archilochus' grandfather, these words have seemed to others to make him his great-grandfather.[200] Perhaps both possibilities should be admitted.

It has been widely assumed that Tellis and Kleoboia were linked in life as well as in Polygnotus' painting, even that they were married.[201] Such assumptions arise from the idea that if they appear together in Charon's boat, they must have some historical connection. But Pausanias' words imply that he attempted to learn all he could about the two figures, and it may be right, therefore, to conclude that no historical or personal connection between the two || was known at Delphi in his time. Thus it may be hazardous to envisage Kleoboia and Tellis together bringing the cult of Demeter to Thasos[202] (*aut sim.*). As regards their supposed marriage, here we are affected by the textual uncertainty mentioned above, since such an idea would be virtually excluded if one accepted Wilamowitz' sug-

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[195] For the older discussions, see *RE loc. cit.* and Frazer, *op. cit.* commentary *ad loc.* (V p. 373f.); more recently, Pouilloux. *Et. Thas.* III 25f., 330; *Fond. Hardt* x, 22f., 36.

[196] *RE s.v.* Thasos, 1311.

[197] Kondoleon in *Fond. Hardt* x. 36.

[198] Cf. n. 184 above.

[199] Pouilloux. *Et. Thas.* III no. 31, col. I. 6 (p. 269); Catalogue I, Col. 4.47 (opp. p. 262).

[200] See *RE* s.v. Tellis, i, 406. The idea that Tellis should be the great-grandfather was revived by Wistrand, *Fond. Hardt* x. 35f.

[201] E.g. Jacoby, 102 n. 4; M. Treu. *Archilochos*, 250, who thinks that we may draw that conclusion from the description of Kleoboia.

[202] As Pouilloux, *Et. Thas.* III 25; cf. 24 n. 4, for a further development of the idea, i.e. that Tellis was priest of Demeter.

gestion. Strictly the description of Kleoboia as ἔτι παρθένος would also exclude the possibility, if that was her condition when she went to Hades.

A connection between Tellis and the foundation of Thasos is only to be considered if (*a*) he and Kleoboia are historical figures, and (*b*) he is linked with her. In that case, since her religious activity may be comfortably accepted as belonging to the beginnings of the colony's life, Tellis also would be involved in its early history. But then we are faced by very grave difficulties. Either the foundation of Greek Thasos has to be put two, or even three, generations before Archilochus, or we have to envisage the introduction of cults at Thasos from Paros before the Parian colonization. The first possibility is in formal contradiction with the ancient synchronism of Archilochus and the foundation of Thasos, the synchronism which is reflected in the story of the foundation oracle, which, in Oenomaeus' account, was given to Archilochus' father and interpreted by Archilochus himself.[203] So we can confidently conclude that the ancients did not hold the view that the colony was founded two, or even three, generations before Archilochus. As for the second possibility, although Pouilloux has pronounced in its favour,[204] it seems inherently improbable, and it is also in conflict with the clear implication of the Heracles story that the Greek colony was established by force.[205] It is not surprising that, faced with these difficulties of chronology or historical probability, Kondoleon preferred to revert to the old identification of Tellis with Telesicles. We have seen the objections to that solution.

The attempt to give Tellis an historical role in the colonization of Thasos leads inescapably to *aporia*. Even if we refrain from embracing the extreme scepticism of those who argued that Tellis' connection with Archilochus was simply invented by the guides at Delphi, and that his name was obviously made up from the word τέλη,[206] we must surely admit that the interpretation of this passage of

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[203] See above pp. 75ff.

[204] *Et. Thas.* III 25f., 330; *Fond. Hardt* x. 22f., 36. Also Blakeway, it seems; *op. cit.* 49 n. 2. E. Jacobs had already noted that, if the genealogy of Tellis is genuine (and Tellis was connected with Thasos, A. J. G.) then the relations between Paros and Thasos began before the colony was sent out (*Thasiaca* 10).

[205] Apoll. *Bibl.* II. 5. 13; *IG* XIV 1293A, 83-4. Cf. M. Launey, *Et. Thas.* I 215f., followed by Bernard, 143.

[206] For these ideas, see *RE* s.v. Tellis, 1, 406; Frazer, *loc. cit.*

Pausanias is so uncertain that no conclusions about the early history of Thasos should be founded upon it.

So we cannot use Tellis to postulate a foundation date two (or three) generations before the *floruit* of Archilochus. Nor can we argue from Telesicles, who must, regrettably, be removed from the list of the founders of Greek colonies.[207] No longer can we imagine the arrival of Telesicles and his companions at Thasos;[208] we must no longer be surprised at the non-appearance of a Telesikleion.[209] So there is no reason in the literary evidence to put the foundation date of || Thasos one generation before Archilochus. As far as the literary evidence goes, we have only the vague synchronism of the colony and the poet himself.

Is it possible to make that synchronism less vague by using the epigraphical material from Paros and Archilochus' poems? The relevant inscriptions consist of (*a*) the so-called *Monumentum Archilochi*, which is perhaps best called Sosthenes' inscription,[210] and (*b*) the Elitas inscriptions (so named from the stream where they were found).[211] Here too a more illuminating name is Mnesiepes' inscription, but abbreviations such as E_1 col. III, etc. (i.e. Elitas inscriptions, stone I, column III) have established themselves.[212] Both these inscriptions

[207] In proposing this radical change, which seems to me unavoidable on the arguments presented above, I am encouraged by the fact that not all scholars in the past have been happy with the idea of Telesicles as *oikistes* of Thasos. Thus E. Jacobs' omission of any statement to this effect, *op. cit.*, must surely be taken as silent rejection, since H. Hasselbach had written long before "Telesicle, Archilochi patre, duce, ut videtur, Pariorum colonia eo (sc. Thasum) pervenit;" see *De insula Thaso* (Marburg, 1838) 14. E. Meyer thought the oracle absurd; see *Gesch. des Altertums* II (Stuttgart, 1893) 467, repeated unchanged in the second edition (Stuttgart, 1937) III. 434 n. 2. Cf. also O. Reverdin, *Fond. Hardt* x. 29: "pour autant que Thasos ait bien considéré Télésiclès comme son véritable κτίστης."

[208] E.g. Pouilloux, *Et. Thas.* III 34: "Télésiklès à la tête d'une poignée de Pariens se saisit de l'acropole Thasienne etc." Devambez, *Journal des Savants*, 1955, 74: "La maigre troupe des compagnons de Télésiklès etc."; Chamoux, *REG* lxxii (1959) 362: "les problemes que se posaient aux compagnons de Télésiclès etc."

[209] See R. Martin, *BCH* lxxiv (1950) 337-41; lxxi-ii (1947-8) 422; Pouilloux, *Et. Thas.* III 344f.; *Fond. Hardt* x. 29; but note the acute criticism of O. Reverdin, which was not rebutted.

[210] *Archilochus*, ed. Tarditi, pp. 18* f. and T5; *IG* XII Suppl. pp. 212-14 = Diehl, *Anth. Lyr. Graeca*³ (1952) fr. 51; Treu, *Archilochos*, pp. 52-62 (cf. *RE* Suppl. XI, s.v. Archilochos, 139f.). That Sostheus of B. col. IV 14, is an alternative form of the name Sosthenes, employed *metri gratia*, is clear from the family connections of Sostheus/Sosthenes; see *IG* XII. 5 p. 316.

[211] See n. 169 above.

[212] E.g. Treu. *Archilochos*, 40ff. The terminology of *SEG* XV 517, (lapis) A (col.) III etc., is better.

originally belonged to an Archilocheion, a gymnasium or museum devoted to religious practices and to honouring the poet and his works.[213] The Parian Chronicle, some sculptured reliefs, and a well set up grave memorial to the poet also belonged, more or less certainly, to the same precinct.[214]

Sosthenes' inscription may be confidently dated by Sosthenes himself, since he can be placed in an important Parian family, and his date of birth can be calculated as *c.* 126 BC.[215] The date of Mnesiepes' inscription is unfortunately only indicated by the character of the writing. The experts of Parian epigraphy place it in the third century BC.[216] The two inscriptions are in general similar; they both give accounts of Archilochus' life, with excerpts from the poems.[217] We have a very full account of Mnesiepes' motives and methods. After receiving the sanction and detailed orders of Apollo through Delphic oracles, he set up a temenos, called it Ἀρχιλόχειον, and wrote about Archilochus what was handed down by οἱ ἀρχαῖοι and what he found out by his own historical research.[218] Sosthenes, on the other hand, admits his dependence on one predecessor, Demeas, who had written down Archilochus' doings and writings according to a chronology by archons.[219]

Although this Demeas has generally been taken to be an otherwise unknown lost Greek historian,[220] Tarditi's theory that he was another like Mnesiepes and Sosthenes themselves, who had set up an inscription about Archilochus, is perhaps attractive. Sosthenes' inscription became necessary, he argues, because Demeas' inscription had become worn or been destroyed.[221] While that may be uncertain, Tarditi is on strong ground when he points out that Demeas can hardly be the source of Mnesiepes' inscription, because

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[213] It is possible to argue about the precise nature of the Archilocheion, but its character in general is clearly established by Mnesiepes' inscription and several analogous foundations; see Kondoleon, *Fond. Hardt* x. 51-4; J. and L. Robert. *Bull. Epig.* 1955, 178.

[214] Kondoleon, *op. cit.* 46, 52-4; *Charisterion ... Orlandos* (Athens, 1965) I 348ff.; cf. *RE* Suppl. XI, s.v. Archilochos, 141.

[215] A. J. Gossage, *Rh. Mus.* xciv (1951) 213-21.

[216] See n. 169 above.

[217] Tarditi, *PP* xi (1956) 128, sees differences in content and expression. As regards the former, we have to remember that we lack much of both inscriptions.

[218] Mnesiepes' inscription, *SEG* XV 517, A II 1-22.

[219] Sosthenes' inscription, A I. 6ff.; cf. 1ff.

[220] *FGH* no. 502.

[221] *Op. cit.* 128f. His main point is that the verb used to describe Demeas' activity

in that case Sosthenes' would have been unnecessary. The sources of Mnesiepes' information about Archilochus are, therefore, seen by Tarditi as (*a*) the rhapsodes who preserved and performed his poems, and (*b*) a biography composed at Delphi in the classical or late classical period. While such ideas must remain speculative,[222] especially in view of the fragmentary state of both inscriptions, it can hardly be denied that the inscriptions present us with two categories of information, the manifestly mythical, like Archilochus' encounter with the Muses,[223] on the one hand, and, on the other, ‖ narrative based on material from Archilochus' own poems.[224] Since we do not possess all of Archilochus' work, and they did, the authors of these inscriptions can offer us valuable material, but it is surely over-optimistic to imagine that they had any independent, detailed, historical tradition outside Archilochus' own poems.[225]

Two final points about the character of these inscriptions. At least one of their motives, perhaps their chief motive, was to present Archilochus as a brave and honourable citizen[226] (in no doubt intentional contrast to the earlier common opinion that he was thoroughly disreputable). We must therefore suspect that the material was chosen in order to vindicate that point of view. Secondly, both inscriptions are sadly incomplete. The only truly well-preserved part of Mnesiepes' inscription is the beginning, giving us the antecedents of his own activity and the story of the epiphany of the Muses to Archilochus and Apollo's prophecy of his greatness (i.e. A, columns I and II). Thereafter we have only the tantalizing beginnings of the lines. Large parts of Sosthenes' inscription are completely missing, and little of what we have is not seriously defective.

In view of all these factors, there is, in my opinion, little profit to be gained by pursuing the often ingenious, and often wildly specu-

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in the passage referred to in n. 219 above, ἀν]αγέγραφεν, should mean 'inscribed' rather than 'composed'.

[222] Cf. Kondoleon's opposition, *Fond. Hardt* x. 50f.

[223] Mnesiepes' inscription, *SEG* XV 517, A II. 22ff.

[224] This is the standard form in both inscriptions. For the way it is done, see Sosthenes' inscription, A I 40ff., cf. A IV 1; Mnesiepes' inscription, B I 12ff.

[225] For the way in which the biography was based on material in Archilochus' poems, cf. Tarditi, *op. cit.* 132ff.

[226] See Mnesiepes' inscription. B I 1ff. and the suggested restorations in Sosthenes' inscription, A I 2-5. Tarditi asked the pertinent question, what were the honours which Mnesiepes paid the poet, with Apollo's approval (A II 14f., 18f.). His acute reply was that the laudatory and apologetic biography which follows itself constituted the honours mentioned; see Tarditi, *op. cit.* 124.

lative, theorizing which these inscriptions have stimulated.[227] Nevertheless, unsatisfactory though this evidence from Archilochus may be, we at least know that it is in general terms historical from the famous inscription found on Thasos to the Glaukos, son of Leptines, who is mentioned several times in the poems.[228] So, if we confine ourselves soberly to the information about Archilochus and Thasos in the poems and the inscriptions which is beyond serious objection, we may legitimately draw some such picture as follows.

His famous description of Thasos and unfavourable comparison with Siris,[229] a colony settled by Colophonians in the reign of Gyges,[230] imply clearly enough that the colonization of Thasos was new, or at least a recent event. Whether it was from this poem when complete, or from others, the ancients were apparently able to deduce that Archilochus was a settler of the colony at Thasos.[231] The complaint that the misery of all the Greeks had come together in Thasos[232] is most easily interpreted as implying that the Parians had invited colonists very widely, but the || possibility must presumably be

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[227] For some of these ideas, see Pouilloux, *Et. Thas.* III 26f., 30-2; *Fond. Hardt* x. 11-14, 17f.; Treu, *RE*, Suppl. XI, s.v. Archilochos, 150f.; Kondoleon, *Phil.* c (1956) 38 (*Arch. Eph.* 1952, 88) offered something entirely different again.

[228] *SEG* XIV 565; Pouilloux, *BCH* lxxix (1955) 75-86.

[229] Fr. 18 D. (= 17, 18 in Tarditi's edition):

... ἥδε δ' ὥστ' ὄνου ῥάχις
ἔστηκεν ὕλης ἀγρίης ἐπιστεφής.
οὐ γάρ τι καλὸς χῶρος οὐδ' ἐπίμερος
οὐδ' ἐρατός, οἷος ἀμφὶ Σίριος ῥοάς.

The reference to Thasos is assured by the authors who quote the fragments.

[230] Or re-settled; see Strabo VI 264; Athen. XII 523c. The complex tradition is discussed by Jacoby, *FGH* III. B Kommentar, p. 495. On the history of the foundation, see J. Bérard, *La colonisation grecque de l'Italie méridionale et de la Sicile dans l'antiquité* (Paris, 1957) 187-98, and T. J. Dunbabin, *The Western Greeks* (Oxford, 1948) 34f., both of whom conclude that the Lydian attacks must belong to Gyges' reign. The archaeological evidence for Greek settlement from the later eighth century on the neighbouring site of the later Heraclea has led some to conclude that the date of the colonization of Siris should be put earlier; see F. G. Lo Porto, "Civiltà indigena e penetrazione greca nella Lucania orientale," *MA* xlviii (1973) nn. 6 and 731. It is perhaps safer to conclude that there was Greek settlement in the region before the arrival of the Colophonians in flight from the Lydians, and that this explains the literary tradition that these Colophonians settled an existing city. On the archaeological zone of Siris and Heraclea, and the relationship of sites etc., see B. Neutsch, *Herakleiastudien, Arch. Forsch. in Lukanien II* (*Röm. Mitt.*, Ergänzungsheft 11, Heidelberg, 1967) 104ff.

[231] See n. 144 above. Presumably the oracle in Oenomaeus, Euseb. *Praep. Evang.* V 33. 1. was concocted on the basis of this knowledge; see above pp. 78ff.

[232] Fr. 54 D. (= 88 in Tarditi's edition): Πανελλήνων ὀϊζὺς ἐς Θάσον συνέδραμεν.

admitted that his reference here is to mercenary soldiers, putatively employed by the Thasians/Parians against their enemies.[233] In either case it is easier to imagine that such a thought would occur either at the beginning, or early in the colony's existence.

The woes of the Thasians and thrice-wretched Thasos[234] do not offer us anything precise to grasp, but they obviously could be connected with the difficulties with the Thracians attested more than once in the poems and Sosthenes' inscription.[235] As well as fighting, the relations with the Thracians involved different contacts, and it is clear that the poet was conscious of shameful behaviour on the Greek side.[236] While it is possible, or even probable, that some of this contact with Thracians relates to the mainland,[237] in at least one

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[233] Cf. above p. 80 and n. 188.

[234] Fr. 19 D. (= Tarditi 19): κλαίω τὰ Θασίων, οὐ τὰ Μαγνήτων κακά; fr. 129 Bergk (= Tarditi 157): Θάσον δὲ τὴν τρισοϊζυρὴν πόλιν. Cf. the remarks of Pouilloux, *Fond. Hardt* x. 13f., 25. 31 and of Page, Reverdin and others, 80f.

[235] First of all in the notorious shield poem, 6 D. (= Tarditi 8): ἀσπίδι μὲν Σαΐων τις ἀγάλλεται κτλ. Sosthenes' inscription. A I. 40-52, has the longest passage about dealings with the Thracians, but is unfortunately not only fragmentary, but also corrupt (clearly 51f.). Some Thracians are definitely killed (49f.); pure gold is brought for Thracian dogs (. . . εἰς Θάσον, ḳυσὶ Θρέϊξιν δῶρ' ἔχων ἀκήρατον χρυσόν, 48f.). It is tempting to connect these lines with Callimachus, *Aetia* fr. 104 (the murder by Parians of Oisydres the Thracian. which was followed by troubles for Thasos: fr. Οἰσύδρεω Θρήϊκος ἐφ' αἵματι πολλὰ Θάσοιο; commentary, φησὶν Παρίους Οἰσύδρην τὸν Θρᾷκα φονε[ύ]σαντας διαπολιορκηθῆναι· Θασί[οις δ'] ὡς τὸ ἀρέσκον Βισάλταις [ἐ]πιτίμιο[ν τ]ίνειν ἔχρησεν ὁ θεός. The supplements are by R. Pfeiffer, *Callimachus* [Oxford, 1949]. Unfortunately the rest of the commentary is too mutilated to restore). Cf. Pouilloux, *Et. Thas.* III 30f.; *Fond. Hardt* x. 17: and Bernard, 143 n. 5, who rightly emphasizes the uncertainties in the detailed interpretation of this incomplete passage. Also perhaps with fr. 61 D. (= Tarditi 97): ἑπτὰ γὰρ νεκρῶν πεσόντων, οὓς ἐμάρψαμεν ποσίν, χείλιοι φονῆές εἰμεν, where the thousand immediately reminds us of χ⟨ε⟩ιλίους γὰρ ἄν[δρας in Sosthenes' inscription, A IV 22; cf. Diehl[3], note *ad loc.* Finally, fr. 79a D. (= Tarditi 193), the Thracians at Salmydessus, merely shows the poet's awareness of their dangerous characteristics.

[236] Fighting most unmistakably in fr. 6 D. (= Tarditi 8), but presumably also in Sosthenes' inscription, and, if the connection is right, in 61 D. (= Tarditi 97); see previous note. That fragment attests shameful behaviour, and Callimachus. *Aetia* fr. 104, reveals that the god ordered the Thasians (or Parians, with different supplements; see Pfeiffer's ap. crit.) to make amends to the Bisaltai for the killing of Oisydres. Also the gift of pure gold to the Thracian dogs (see previous note) brought general evils, if private gain (οἰκείω⟨ι⟩ δὲ κέρδει ξύν' ἐποίησαν κακά, Sosthenes' inscription, A I 49).

[237] Pouilloux thinks that the struggles attested by Sosthenes' inscription and by Call. *Aetia* fr. 104 took place on Thasos rather than the mainland; see *Fond. Hardt* x. 18. But we know that the Thasians were already interested in the mainland in Archilochus' lifetime; see Pouilloux. *Et. Thas.* III 33 (the clearest evidence is Philochorus *FGH* 328 frg. 43 = Harpocration s.v. Στρύμη). Further, since we have the names

passage it is more natural to take the Thracians mentioned ('Thracian dogs') as on Thasos itself.[238]

The probable involvement of the Parians (as opposed to the colonists) in these doings might be held to make it likely that the events are near to the date when the colonists left Paros.[239] It also seems probable that Archilochus did not stay permanently in Thasos,[240] and that might indicate that he was able to take advantage of a right to return to Paros. In at least one case, at Cyrene, such a right was only accorded the colonists for five years from their departure.[241] But we are on uncertain ground here; in other instances more easy conditions for return are known.[242]

There is nothing in this evidence which proves that Archilochus did not participate in the original colonial expedition to Thasos. Since the oracle cited by Oenomaeus of Gadara (Euseb. *Praep. Evang.* v. 31. i), 'Αρχίλοχ' εἰς Θάσον ἐλθὲ καὶ οἴκει εὐκλέα νῆσον, is not authentic, we cannot build any chronological theories on a supposed contrast between it and the oracle (also || concocted) given to Telesicles.[243] It seems to me perfectly possible, and even likely, that Archilochus' participation in the affairs of Thasos was at the beginning of the colony's history. However, our evidence is not sufficient to exclude the possibility that he formed part of a subsequent reinforcement, as has regularly been assumed.

The conclusion from this discussion of the literary evidence must therefore be that there is no true literary date for the foundation of Thasos. We can do no better than the ancients who made the synchronism Archilochus-Thasos, and that synchronism, as we have seen,

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of two, if not three, Thracian tribes in these events (Saioi. fr. 6 D., Bisaltai, Call. *Aetia* fr. 104 [Commentary], and Sapai [?], Sosthenes' inscription, A I 51), it might be thought difficult to locate all the contacts on Thasos itself. There is the additional point that the Thracians on Thasos were probably Edonians; see below n. 253.

238 See n. 235 above.

239 See my *Colony and Mother City in Ancient Greece* (Manchester, 1971) 72f.

240 This seems to be indicated by Critias, fr. 44 (see n. 144 above); by his death in a war presumably between Paros and Naxos (since his killer, Carondas the Crow, was a Naxian; see Aelian, fr. 80 [Teubner]; Plut. *Mor.* 560 D-E; Dio. Chrys. xxxiii. 12); and by his grave on Paros (see p. 83 and n. 184 above). It is true that a sceptic might doubt the worth of all these indications. On Archilochus' possible movements, see Jacoby 109.

241 *SEG* IX 3. 33-7; see my *Colony and Mother City*, 53.

242 As at Naupactus; see *ML* 20 and my comments, *op. cit.* 52f.

243 As Pouilloux, *Fond. Hardt* x. 9f.; cf. *Et. Thas.* III 26. On the oracles, see above pp. 78-80.

must be approximate. With the help of the Gyges poem and the Woes of the Magnesians we can give an absolute date (though obviously only in round terms) of *c.* 650 to Archilochus' activity.[244] Therefore, on the literary evidence, the date of the foundation of Thasos should be *c.* 650, or perhaps earlier.

Historical discussion

This conclusion removes the one serious obstacle which appeared to conflict with the obvious interpretation of the finds from the two sondages in Champ Héraclis and Champ Dimitriadis. The date achieved for this archaeological material by Bernard (i.e. *grosso modo* first half of the seventh century) no longer requires the complicated and historically improbable notions put forward to take account of the supposed chronological relationship between that date and a literary foundation date of *c.* 680 (or first quarter of the seventh century) for Parian Thasos.[245] We may now confidently revert to the convincing interpretation of Salviat,[246] and see the material from the two sondages as precolonial in the full sense.

The archaeological date for the arrival of the Parian colonists can now be sought unhampered by the restrictions imposed by what seemed to be a secure literary date. For it is clear from our consideration of the literary evidence that the date indicated by that evidence is no closer than one which we might hope to obtain from the material finds. That archaeological date for the Parian colonization is, therefore, to be found in the earliest Greek orientalizing, and especially Cycladic, finds from the site, i.e. the finds which led to such formulations as 'cette croissance vigoureuse' by those who conceived of a foundation date about a generation earlier.[247]

There is general agreement among the experts that the earliest such ceramic material is of about the middle of the seventh century, or a little earlier.[248] As we are confined to painted pottery dated

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[244] Jacoby, 97-109, especially 106f.; though he presses the evidence unnecessarily.

[245] See above pp. 70ff. On the other reason for regarding the culture discovered in these sondages as Greek—Bernard's identification of the apsidal house as Greek in type—see above pp. 65f. and 71.

[246] See above p. 70 and n. 88.

[247] See above p. 71.

[248] J. Boardman, *Greeks Overseas*², 225; Bernard, 77; Pouilloux, *Fond. Hardt* x. 14, 17.

largely by style,[249] a precise date is not attainable, but an interesting argument in || favour of *c.* 660 was advanced by Kondoleon.[250] He, like Salviat,[251] regarded the so-called 'Melian' pottery as really Parian, and he thought that the mass of such 'Melian'/Parian vases begins *c.* 660. He also favoured the view[252] that the subgeometric pottery called 'Siphnian' is in reality an older phase of 'Melian', hence also Parian. None of this 'Siphnian' pottery has been found on Thasos. So he suggested that the archaeological date for the Parian colonization of Thasos lies at the break between these two phases of Parian painted pottery, i.e. on his dating *c.* 660. There

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[249] Cf. the judicious discussion by N. Weill and F. Salviat, *BCH* lxxxiv (1960) 382-6, lxxxv (1961) 120-2. The oriental ivory lions found in the so-called west fill of the Artemision have been used for chronology; see Pouilloux, *Fond. Hardt* x. 16: "Ils datent, au plus tard, du deuxième quart du VIIe siècle etc." But the full publication by Salviat, *BCH* lxxxvi (1962) 95-116, shows that these ivories are, in fact, useless for chronological purposes. They were found in a fill laid down in the second quarter of the fifth century (*BCH* lxxxiii [1959] 775), consisting of archaic dedications going back to the seventh century (*BCH* lxxxvi [1962] 112). Salviat argued, 112-16, that the analogous ivories found in the east did not preclude the possibility that the Thasian ones were current in the second quarter of the seventh century (i.e. after the supposed foundation of Parian Thasos in 680); hence Pouilloux' statement. But, far from having a good independent date for the ivories, Salviat was using the presumed foundation date of Thasos to throw doubt on the high dates proposed by some for similar ivories found in the east. On such ivories in general, see M. E. L. Mallowan. *Nimrud and its remains* II (London, 1966) 471ff., and Irene J. Winter, "Phoenician and North Syrian ivory carving in historical context," *Iraq* xxxviii (1976) 1-22. Dr. Winter (to whom I am most grateful for helpful discussions of this question) tells me that in her opinion the best parallels for the Thasian ivories are at Zincirli and at Samaria (in this agreeing with Salviat, 100ff.). She would assign them to a postulated South Syrian group (this in addition to the North Syrian and Phoenician groups distinguished earlier; cf. Winter *ibid.*), and date them to the eighth century BC. Dr. Winter intends to develop the hypothesis of the South Syrian ivories in a forthcoming publication.

Viewed purely as dating evidence, the remains of such a luxurious object (? an ivory throne), far removed from any chronological context in which it had existed in its original form, are manifestly valueless. Who can tell how this sumptuous object reached the Artemision? Or how old it was when it came there? But it seems to me likely (as against Salviat, 112) that it was a valued antique rather than a contemporary production when it was dedicated to Artemis.

[250] *Fond. Hardt* x. 60.

[251] *VIIIe Cong. Arch. Class.* 300f. Boardman, however, argues with some sarcasm against a change of title. In his view the attribution to Paros is no better founded than the title 'Melian'; see John Boardman and John Hayes, *Excavations at Tocra 1963-5, The Archaic Deposits* I (*BSA* Suppl. 4, London, 1966) 15. In taking this line he differs not only from Kondoleon, Salviat, and others, but also from his fellow-author; see p. 73. For the uncertainty in attributing pottery from the Cyclades, see *Tocra* II (*BSA* Suppl. 10, London, 1973) 73f.

[252] See especially J. K. Brock, *BSA* xliv (1949) 79.

are three uncertainties in this argument, not to mention the fact that it is also based on absence of finds on Thasos, but it remains interesting and could be correct.

The archaeological date for the foundation of Parian Thasos, according to our present knowledge, is, therefore, about the middle of the seventh century, or a little earlier. Thus it coincides perfectly with the best literary date that we can at present achieve, and these two dates are reached entirely independently. Both dates could well be changed or refined by new evidence, because neither is so solidly or precisely established as to be invulnerable. Until such new evidence appears, however, we can say that we have a clear, if approximate, date for the establishment of the Greek colony. As we have seen, this date also allows us to make a clear distinction between the material from the precolonial settlement and that from the subsequent Greek colony. The historian can therefore use this evidence as a basis for some reconstruction of pre-Greek Thasos and of the circumstances and character of the Parian colonization.

The literary evidence leaves no room for doubt that the pre-Greek inhabitants of Thasos, of whom we now possess some scanty memorials from the site of Thasos and elsewhere in the island, were a branch of the Thracians.[253] This literary evidence is now confirmed by archaeological finds. As we saw above, pottery from Kastri is comparable to the Knobbed Ware of Troy VIIB, which is recog-

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[253] Cf. Pouilloux *Et. Thas.* III 15f. For Archilochus' evidence of Thracians on Thasos, see above p. 85. The fact that an earlier name for Thasos was Ὀδωνίς (Hesych. s.v.) is generally connected with the Edonians; see H. Seyrig, *BCH* li (1927) 216. In his ninth labour Heracles overcame the Thracian inhabitants and gave the island to the sons of Androgeos, whom he had brought from Paros; see Apoll. *Bibl.* II 5. 9. 13; cf. 3-5. Compare Ps-Scymnus, 659ff.: νῆσος Θάσος, ἣν βάρβαροι τὸ πρότερον ᾤκουν, ὡς λόγος, ἔπειτα Φοίνικες κτλ. On the other hand, it is perhaps incorrect to take the passage Strabo XII 550 as referring to Thasos (as, e.g., Pouilloux, *Et. Thas.* III 16). Strabo says that the Thracians called Sinties, then Sintoi, then Saioi, but now Sapaioi, used to live about Abdera and the islands around Lemnos. But the comparison with the very similar passage X 457, where he is discussing the population of Samothrace, may suggest that the islands about Lemnos could be, for Strabo, no more than Lemnos, Imbros, and Samothrace. (That would imply that Strabo thought of Archilochus' fighting with the Saioi as taking place on the mainland, for he quotes the shield poem in both passages, but, as we have seen, that is perfectly possible [above, n. 237]. Also, if the previous name Odonis is to be connected with the Edonians, as seems probable, the Thracian inhabitants of Thasos should rather be taken as part of that great Thracian tribe, who lived in the Pangaean district; see *RE* s.v. Edones; Seyrig, *loc. cit.*)

nized to be a Thracian settlement.[254] In addition, hand-made local pottery from a site on the opposite mainland near the modern Pontolibadon, a site which Mme. Koukouli-Chrysanthaki identifies as Pistyros, is very similar to the pottery in the sondages in Champ Dimitriadis and Champ Héraclis.[255] Since the mainland settlement will manifestly have been Thracian, this pottery confirms the identity of the settlement at Thasos. ||

We now know, therefore, that the Thracians preceded the Greeks in occupying at least part of the site of the Greek polis of Thasos. Their settlement seems to have been fairly extensive, if we may argue from the fact that it has been found in two sondages *c.* 200 m. apart. It lay between the harbour(s) and the acropolis in an area which was also occupied by houses in the later Greek city.[256] Apart from the knowledge that they lived in fairly simple and primitive houses, of apsidal or oval shape, with walls of rough stone and roofs of reeds and clay,[257] we have only ceramic evidence from which to deduce anything about their way of life.

This pottery has been discussed fully above.[258] These Thracians probably made some coarse hand-made ware for kitchen use, but they imported their table ware from the mainland to the north and from places which cannot be exactly determined in the Aeolian area to the south-east. In addition to this table ware, some of the imported pottery is of containers, almost certainly wine amphorae. This wide range of imported pottery is one of the most interesting features of the settlement, and allows us to make a number of inferences about it.

First of all, it seems likely that the famous vineyards of Thasos did not exist at this period. All the pottery connected with wine (these amphorae, the Macedonian oenochoae probably, and many sorts of drinking vessels) is imported. One is reminded of the way in which the people of Celtic lands and the Scythians imported their wine and the vessels with which to serve and drink it.[259] Archilochus'

[254] Above pp. 71f.
[255] *AAA* vi (1973) 239f.
[256] See above pp. 62f.
[257] See above p. 65.
[258] Pp. 66ff.
[259] Cf. F. Villard, *La céramique grecque de Marseille* (Paris, 1960) 119ff., and P. Alexandrescu, RA 1975, 63-5, who discusses early examples of oenochoae and cups found in the valleys of the Dnieper and the Don. For early imports of amphorae

evidence also suggests that the famous Thasian wine was not yet. Plutarch accuses him of ignoring the famous vineyards in his unfavourable description of the appearance of Thasos.[260] It is better to assume there were none to describe. The wine that Archilochus was drinking came from Ismaros, near Maronea, on the mainland.[261]

If the Thracians on Thasos were able to import their pottery and their wine, they must have had some goods to supply in exchange. We know that in later times Thasos had rich resources of agricultural products, fish, timber, but above all minerals.[262] We can decide which of these was most likely to have provided these Thracians with their prosperity by asking a further question. Who brought these goods of widespread origin to the harbour at Thasos? If we may trust Herodotus the reply is obvious. It was the Phoenicians. And the Phoenicians came for minerals.

There are two passages in Herodotus which provide information about the Phoenicians and Thasos, ii. 44. 3-4 and vi. 47. 1-2. In the first he says that the temple of Heracles on Thasos was established by the Phoenicians, who sailed off in search of Europa and founded Thasos. This he supports with the statement that in Tyre there was a temple of Heracles called Thasios. In the second he says that he had seen the gold mines (of Thasos and the opposite mainland), and that the most remarkable were those discovered by the Phoenicians who settled the island with Thasus, whose name the island now bears. These mines where, he tells us, situated between the places called Ainyra and Koinyra, opposite Samothrace, and a great mountain had been utterly turned over in the search (i.e. for gold).

There is a long list of modern writers who have asserted that there were no mines where Herodotus places them, though clear traces of mining activity observed in the area have been reported in publications going back to 1909.[263] It is true that there was, until 1964,

in the Dnieper region, see N. A. Onaiko, *Antichny Import v Pridnieprov'ie i Pobuzh'ie u VII-V vekakh do n.e.* (Moscow, 1966) 58f.

[260] *Mor.* 604 *c.*

[261] Fr. 2 D. (= Tarditi 2). Cf. *RE* s.v. Maroneia, and s.v. Ismaros, 3. Possibly the fame of the wine of Ismaros in Homer is enough to account for Archilochus' words, but one recalls the interest of the Thasians in that part of the mainland in Archilochus' lifetime; see above n. 237.

[262] On the resources of Thasos in general, see H. de Coincy. 'L'île de Thasos', *La Géographie*, xxxviii (1922) 405-26, and more recently, D. Lazaridis, *Thasos and its Peraia* (Athens, 1971). Cf. also Pouilloux, *Ét. Thas.* III 13.

[263] See the excellent bibliographical survey in Salviat and Servais, *BCH* lxxxviii (1964) 267-87, especially 282f. See nn. 9 and 10 above.

one uncertain ‖ element in Herodotus' topographical indications. While Koinyra has long been indentified with the modern Kinyra, Ainyra could not be placed with certainty. Since the publication of the delightful signpost inscription by Salviat and Servais, there has been no doubt that Ainyra was the ancient name of the modern Potamia.[264] Thus the mountain between Ainyra and Koinyra facing Samothrace can only be the eastern spurs of Mount Hypsarion. And in these eastern spurs, on the SE. slope of the subsidiary peak called Phanos, there are abandoned galleries, piles of stones, and other debris, sherds of amphorae, etc., i.e. unmistakable evidence of mining activity.[265] The position of the mines seen by Herodotus may, therefore, be regarded as certainly identified.

As to Herodotus' statement that these mines were discovered by Phoenicians, Salviat and Servais argued that it is confirmed by the fact that Ainyra and Koinyra are semitic names, and contain elements connected with gold and silver. Some of their argument seems unconvincing, but the connection of Koinyra with the names Kinyras (king of Cyprus in the Iliad) and Kinyreia (the Cypriot placename), both of which are generally thought to be semitic, seems very likely.[266] There should surely no longer be any doubts about accepting the information given by Herodotus in vi. 47.

A Phoenician origin for Thasian Heracles has also frequently been denied by modern experts,[267] but the case for accepting the ancient tradition has been argued by van Berchem with such persuasive learning, skill, and imagination,[268] that I find it difficult to imagine that it will ever again be doubted. It will be sufficient here to indicate the main lines of his case.

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[264] *Op. cit.*; see especially 276-82. For the inscription cf. also *Bull. Epig.* 1965, 316.

[265] Salviat and Servais, *op. cit.* 282f.

[266] See Salviat and Servais, *op. cit.* 284. On Kinyras, Kinyreia etc., see *RE* s.v. Kinyras. I am grateful to Dr. T. Muraoka of the Department of Near Eastern Studies, University of Manchester, for expert advice on this matter.

[267] The opinions on the subject down to that date were usefully set out by H. Seyrig, *BCH* li (1927) 186 n. 1. For a brief indication of the views of scholars since that time, see van Berchem (next note), 88.

[268] "Sanctuaires d'Hercule-Melqart, contribution à l'étude de l'expansion phénicienne en Méditerranée,' *Syria* xliv (1967) 73-109, 307-36. For Thasos, see especially 88-109. It was very unfortunate that Bergquist wrote her study of Herakles on Thasos without knowledge of van Berchem's work; see Birgitta Bergquist, *Herakles on Thasos* (Uppsala studies in Ancient Mediterranean and Near Eastern Civilizations, 5, Uppsala, 1973), especially p. 18: "Apart from Pouilloux's work, there seems to

We know that the Phoenician trading settlements were generally established side-by-side with the community with whom they were dealing, and that a regular feature of such settlements was the establishment of a place of worship for the Phoenician deities.[269] The cult of Heracles Melqart is particularly well known, as it was practised at Gades, so we can use the practices and institutions attested there as a touchstone in discussing other possible occurrences of the same cult.[270] At Thasos van Berchem is able to point, first of all, to the clear ancient tradition.[271] We are fortunate to know some at least of the rules of the cult of Heracles at Thasos. The ban on the participation of women and the ban on the sacrifice of pigs and goats are particularly suggestive, when compared with what we know at Gades and elsewhere.[272] The sanctuary itself is also revealing. It is centred on the great altar and there is no sign of a house for the god. Among other significant points of comparison with Phoenician ritual, van Berchem ingeniously infers that there was no image of the god to be housed in a temple in the Greek fashion, i.e. the cult was aniconic, as in Semitic religions.[273] In view of all this, it is perhaps not surprising, though it is most striking, that the Thasian festival of the *Soteria*, which is confidently to be interpreted as a festival of Heracles Soter, occurred in the same month as the great festival of Melqart of Tyre.[274] The discoveries and arguments of Salviat and Servais about the goldmines provided van Berchem with a supporting argument.[275] He concluded that the cult was established by the Phoenicians before the Greeks colonized Thasos, and the practices of the

have been no particularly extensive discussion of Herakles and the Herakleion on Thasos. . . ." However, her independent arguments from the material remains point in general to similar conclusions to those reached by van Berchem, in particular that there were not two cults, one of the god, one of the hero, at the Thasian Heracleum.

269 Van Berchem, 74-7.

270 Van Berchem, 80-7.

271 P. 88. Apart from Hdt. II 44. 3-4, see Paus. V 25. 12.

272 Van Berchem, 87-102. In this part of his work he deals admirably with the complexities in the evidence for the cult of Heracles at Thasos. Cf. also Bergquist, *op. cit.*, especially Part II, but van Berchem's interpretations seem to me in detail superior (e.g. on the meaning of ἐνατεύειν; Bergquist 71-80, van Berchem 100-2).

273 See pp. 92-4, 97f.; cf. 84. Bergquist, however, thinks that the peripteral building erected in her 'third period' (i.e. mid fifth to late fourth or early third century BC) was a temple to house the god's image; see *op. cit.* 47.

274 Van Berchem, 102. Bergquist, *op. cit.* 37, also thinks that the Soteria was probably a festival of Heracles.

275 Pp. 106f.

cult, being rooted in the indigenous population, were taken over by the Greeks and hellenized, though not sufficiently to conceal their Phoenician origin.[276]

There is a further interesting point, made by Devambez,[277] which strengthens van Berchem's argument. The Heracleum at Thasos is a long way, 400 metres and more, from the area under the acropolis which has been seen as the first area settled by the Greeks (and which we now know was also the area settled by the Thracians who preceded the Greeks). Devambez thought that this position might well indicate that the cult of Heracles was precolonial, already established when the Greek colonists came.

Van Berchem also suggested that we can trace the route followed by the Phoenicians who came to Thasos, since it is marked out by the stations of Heracles, i.e. Cyprus, Cos, Erythrae, and Lemnos.[278] The last of these is highly suggestive in view of the pottery from the precolonial settlement.[279] The evidence for the worship of Heracles on Lemnos is not sufficiently known for any decision to be made as to whether it was a Phoenician cult.[280] But it is not necessary for our purposes to come to such a decision, since the presence of the Phoenicians on Lemnos is clearly attested by Homer.[281] We have no difficulty, therefore, in understanding how the Lemnian pottery reached the precolonial settlement at Thasos.

The presence of the Phoenicians on Cyprus is well established,[282] but is perhaps hardly relevant, as our evidence stands, to their activities in the North Aegean. On the other hand, the evidence about the cult of Heracles in Erythrae and in Cos is worth mentioning in this connection.

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[276] P. 108. Cf., for the long preservation of cult practices, even when populations change, the arguments and evidence on pp. 77-9. Bergquist, who accepts the notion of Phoenician colonization of Thasos before that of the Greeks (*op. cit.* 19, 35), wonders if the cult of Heracles on Thasos may have had Phoenician elements (37).

[277] P. Devambez, 'Questions thasiennes', *Journal des Savants*, 1955, 28-40, 73-91; see especially 84.

[278] P. 108.

[279] See above p. 67.

[280] The inscriptions cited by van Berchem (108 n. 4) show us merely that there was a temple of Heracles on the island from the fourth century BC (*IG* XII 8. 18-22, notably 19).

[281] *Il.* xxiii 745; cf. *RE* s.v. Thoas (2).

[282] For a brief, general, account, see Donald B. Harden, *The Phoenicians*, (London, 1971) 52-4, 109. For a fuller discussion of the topic, and the evidence down to that time, E. Gjerstad, *Swedish Cyprus Expedition* IV. 2 (Stockholm, 1948) 436ff., 462. For a more recent statement, V. Karageorghis, *Kition* (London, 1976) 95ff. Cf. also

For Erythrae we have a curious story in Pausanias (vii. 5. 5-8). The image of Heracles at Erythrae, he says, is absolutely Egyptian.[283] The god set out on a wooden raft from Tyre. When the raft reached Ionian waters it stopped at a cape called Mesate, midway between Erythrae and Chios. Both the Erythraeans and the Chiots sought to land the image on their shores. A blind Erythraean fisherman had a dream that a rope made from the hair of the women of Erythrae would enable the men to tow the raft to their land. When the Erythraean women refused to cut off their hair, the Thracian slave-women and freewomen living in Erythrae cut off theirs. A rope was made and the raft and image were towed ashore. No women except Thracians are allowed to enter the sanctuary, Pausanias reports, and the rope of hair was preserved till his time. ||

That the origins of the cult were Phoenician, as this myth indicates, need not be doubted. The tale of Heracles on the raft is shown to be Phoenician by the scarabs which depict the scene and bear symbols of unmistakably Eastern significance.[284] The part played by the Thracian women in the story is clearly aetiological; it is to explain (*a*) their special privileges at the sanctuary, and (*b*) the rope of hair preserved there.[285] We have insufficient information to reconstruct with any conviction precisely what cult practices lie behind these facts, as the experts on Greek religion have had to admit.[286] The rope of hair is taken to imply actual dedication of hair, and that could symbolize the dedication of chastity, i.e. we are perhaps dealing with temple prostitution.[287] The role of the Thracian women (the slaves at any rate) would thus be easily explained. But it is not profitable to pursue such speculations further. For our present purpose it is enough to note that Thracian women had a special place in a cult of Phoenician origin in Erythrae. It is not too bold a con-

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Coldstream, *Greek Geometric Pottery*, 347 n. 2, and *Bull. Inst. Class. Stud.* xvi (1969) 6 n. 14.

[283] For the probable character of the image, see Frazer, *Pausanias*, commentary *ad loc.* (IV 127f.).

[284] E. Courband, 'La navigation d'Hercule', *Mél. Arch. Hist. Ecol. Franç. Rome*, xii (1892) 274-88, and Frazer, *ibid.* L. R. Farnell cast doubt on the interpretation without any strong arguments; *Greek Hero Cults and Ideas of Immortality* (Oxford, 1921) 164.

[285] See Frazer, *ibid.*; Farnell, 161.

[286] Farnell, 160-5, whose own interpretation is vitiated by the lame suggestion that Thracian was merely an indication of servile status; cf. W. R. Halliday, *Greek Questions of Plutarch* (Oxford, 1928) 215.

[287] Frazer, *ibid.*; cf. Farnell, 163.

jecture to suggest that Thracian women were once brought as slaves to Erythrae by Phoenicians trading with Thrace. One recalls what Herodotus says about the Thracian practice of selling their children into slavery abroad (v. 6. 1), and the reputation of the Phoenicians as slavers (*Od.* xiv. 287ff.; xv. 415ff.).

Our information about Cos comes from Plutarch (*Q.G.* 58), who discusses the question why the priest of Heracles at Cos wears women's clothing at Antimacheia, and puts on a woman's headdress to perform sacrifice. Plutarch explains these customs, not very exactly or logically,[288] by a detailed story of what happened to Heracles when he was shipwrecked in the region. In these adventures he was saved by a Thracian woman and by disguising himself in female apparel. Van Berchem is able to show by comparison with cults at Gades and Rome, that the custom whereby a priest wore women's clothing shows that the cult was originally of Heracles/Melqart.[289] The appearance of the Thracian woman, totally unexplained in Plutarch's passage, has been seen as implying some privilege for Thracian women in the cult of Heracles at Cos, which gave rise to this part of the aetiological myth.[290] Naturally the comparison with Erythrae is regularly made. Once again we have only a fragment of the information we need, but the same historical circumstances seem to be implied as at Erythrae.

At the least this discussion shows that the route which van Berchem briefly indicated is plausible, and the connections of the Phoenicians with Thrace provide a little further support for the conclusion that the trade with precolonial Thasos was in their hands.[291]

It might be objected that there is a lack of archaeological confirmation of a Phoenician presence in the North Aegean at the relevant time. However, an oenochoe from Troy, Settlement VIII, is clearly a copy of a common Phoenician shape, the piriform oenochoe,[292] and it is not unreasonable to imagine that the ivory throne mentioned in note 249 above was brought to Thasos by Phoenicians.

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[288] See the criticisms of Farnell, 160; Halliday, 215.
[289] Van Berchem, 108; cf. 86, 317ff.
[290] Farnell, 161.
[291] Apart from the specific evidence and arguments offered, such a conclusion would be concordant with the general historical picture of Phoenician trading activity from the ninth to the seventh centuries; cf. Muhly, 'Homer and the Phoenicians', *Berytus* xix (1970) 19-64.
[292] *Troy* IV pl. 292, inventory number 36.722, 317 no. 27; see text p. 265.

We may conclude (with all necessary reserves) that before the Parians came to Thasos, it was occupied by a Thracian community who had close commercial (and perhaps other) relations || with the Phoenicians. Although we know nothing about the political circumstances of this community, and almost nothing of its culture, it was clearly successful in economic terms. Both the natives and the Phoenicians were, it seems, profiting from the latter's exploitation of the valuable mineral resources of the area. If this is, approximately, the correct picture of precolonial Thasos, how does it affect our ideas about the Parian colonization of the island?

The first question must obviously be: was the Parian settlement achieved peaceably or by force? The view that the relations between the existing Thracian inhabitants and the Parian newcomers were fundamentally good and friendly was very fully developed by Pouilloux.[293] He argued that not only was Thasos a mixed city in which the Thracian element was substantial and important, but that this state of affairs went back to the beginning of the Parian colony, when the colonists made an alliance or agreement with the native Thracians.[294] The fundamental basis for this argument was Seyrig's thesis that the onomastic evidence shows that leading Thasian families had intermarried with Thracians at the beginning of the colony's existence.[295] However, Pouilloux also regarded Thracian influence on cults at Thasos as supporting evidence.[296] He also suggested, writing before any prehellenic material had been discovered by the archaeologists, that the absence of material evidence for pre-Greek settlers on the site of Thasos showed that the Greeks had not *superimposed* themselves on the natives, but had rather *juxtaposed* themselves beside the previous inhabitants.[297] This last argument is now known to be invalid.

Pouilloux was criticized by Devambez,[298] who denied that any of the religion or customs of Thasos are Thracian or mixed. In his

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Compare P. Cintas, *Céramique Punique* (Paris, 1950), pls. XV, LXXXI; J. M. Blazquez, *Tartessos y los origenes de la colonizacion fenicia en occidente* (Salamanca, 1968) 169ff., pls. LXIV, LXV. I owe this important observation to Mr. Brian Shefton.

[293] *Et. Thas.* III 30f., 34; *Fond. Hardt* x. 15f.

[294] *Et. Thas.* III *ibid.*, and 16f., 21, 311-13.

[295] 'Quatre cultes de Thasos', *BCH* li (1927) 178-233, especially 217-19; cf. Pouilloux, *Et. Thas.* III 311-13.

[296] *Et. Thas.* III ch. viii, 326ff.; cf. 21.

[297] *Et. Thas.* III 15f.

[298] See n. 277 above.

view the mixed city does not appear before the beginning of our era. Even so, Devambez accepted Pouilloux' thesis that there was intermarriage between the Greeks and Thracians and that peaceful relations were quickly established.[299] Pouilloux was also criticized by Chamoux,[300] who reduced the number of names taken by Pouilloux to be Thracian,[301] and thought that the few Thracian names known do not justify any such large conclusions as a mixed Greco-Thracian city. For Chamoux the notable thing is the very small importance of the non-Greek element at Thasos.[302]

These criticisms seem to be well-founded, and we may firmly set aside the notion of a mixed Greek and Thracian city. But there still remains the question of intermarriage at the time of the colony's foundation. Seyrig's argument in favour of such a hypothesis was the result of his acute observation that, in the very numerous pairs of names in the lists of magistrates of Thasos from the late sixth to the fourth century, the Thracian names are invariably linked with Greek names; i.e. a Thracian name is always attached to a Greek patronymic and a Thracian patronymic to a Greek name. He concluded that the Thracian names were artificial (i.e. borne in fact by Greeks), and they persisted in certain families in memory of the marriage alliances which had been made with great barbarian families at the moment of colonization.[303]

Seyrig's contention that the Thracian names in these pairs must be artificial is undeniable, but it is surely quite unnecessary to go on to conclude that they reflect marriage alliances made at the moment of colonization. As Seyrig himself saw,[304] we possess in Thucydides an ideal analogy by which to interpret these artificial Thracian names. Thucydides' patronymic Oloros is, we need not doubt, an artificial name given to his father in memory of his ancestor, the Thracian king Oloros, father of Hegesipyle, second wife of the Younger Miltiades.[305] The patronymic, therefore, indicates a

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[299] *Op. cit.* 73f.

[300] 'L'isle de Thasos et son histoire', *REG* lxxii (1959) 348-69.

[301] See pp. 350f.

[302] *Ibid.*

[303] See n. 295 above.

[304] *Op. cit.* 218.

[305] On Thucydides and his family, see Cavaignac, *RPh* lv (1929) 281-5; Wade-Gery, *JHS* lii (1932) 210f.; Gomme, Commentary *ad* IV 105. 1; O. Luschat, *RE* Suppl. XII, s.v. Thukydides, 1089ff.; J. K. Davies. *Athenian Propertied Families 600-300 BC.* (Oxford, 1971) 233-7. It is possible to argue about details, but the general scheme

marriage alliance between a Greek and Thracian family, but a marriage alliance contracted in the late sixth century. It is also noteworthy that, as a result of this marriage alliance (again we need not doubt) Thucydides owned a mining concession in the Pangaean area.[306] Who would have been more likely to have contracted such valuable marriage alliances with Thracian families than leading men of Thasos?[307]

Our first definite examples of these artificial Thracian names at Thasos are of the fifth century,[308] so there is no justification for supposing that they attest marriage alliances between Thasian and Thracian families earlier than, say, the sixth century. If we are right that the main attraction to the Greeks in such marriage alliances would be mining concessions and property rights in areas controlled by Thracians, it is obvious that the Thracians concerned would be Thracians of the mainland. For both these reasons, chronological and geographical, the few Thracian names that are attested in leading families at Thasos tell us nothing about the colonization of the island.

Intermarriage between Greek colonists and Thracian women was proposed by Hiller von Gaertringen in an attempt to restore and interpret a fragmentary passage of Sosthenes' inscription, A IV 22f.

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proposed by Cavaignac seems convincing. In view of the clear evidence for relationship between Thucydides and the Philaids, to explain the patronymic other than as in memory of the Thracian king seems an unnecessary multiplication of hypotheses.

[306] The *Life* thought his mining possessions came through his marriage to a rich Thracian; 19. But it is notorious that much of what we have in the *Life* is invented on the basis of information in Thucydides' own work. See *RE loc. cit.* 1087. It is also suspicious that the biographer gives him actual property in Thrace, in contrast to the mining concession carefully specified by Thucydides himself, IV 105. I, as it is also suspicious that his mine should have been located at Skapte Hyle (14, 25, 47; cf. Plut. *Mor.* 605 c), the only place-name known in the mining district (Hdt. vi 46). So this information in the *Life* has been rejected as invented (see *RE loc. cit.* 1096). The idea that Thucydides' concession came as a result of Athens' appropriation of the possessions of Thasos (as still Davies, *op. cit.* 236f.) is surely wrong. Our evidence suggests that such concessions were the result of private arrangements with the Thracian owners (*RE loc. cit.* 1095f.). It is commonly objected to the theory that Thucydides' concession was an inheritance from the Thracian king Oloros that we then have the problem of a Thracian king in the Chersonese owning property in the Pangaean area (*ibid.* 1089f.). But nothing in our evidence requires us to see this as a problem, and it is not even certain that Oloros was at home in the Chersonese; see Cavaignac, *op. cit.* 284.

[307] Cf. Thuc. I 100. 2; *RE loc. cit.* 1095.

[308] Pouilloux, *Et. Thas.* III 311f., as corrected by Chamoux, *REG* lxxvii (1959) 350f.

He cited Herodotus' statement about the Ionian colonists and the Carian women (i. 146. 2-3), and suggested that

> χ⟨ε⟩ιλίους γὰρ ἄν[δρας . . .]
> [ἔ]πειτα γυναῖκας εἶ[χον . . .]

be understood to mean that a thousand colonists were sent who took Thracian women as their wives.[309] This could be right, though it is very bold, and Herodotus' story is manifestly aetiological. Clearly nothing could be built on such uncertain foundations and, in any case, if we do apply the Herodotean analogy, we are not envisaging good relations between the colonists and the natives, but the reverse.

The famous memorial of Glaukos, son of Leptines,[310] has also been interpreted as indicating good relations between Parian colonists and native Thracians, on the grounds that οἱ Βρέντεω παῖδες were Thracians, because Brentes is a Thracian name.[311] The weakness of this etymological argument is that the Thracian names and words which are compared with Brentes can easily be matched by perfectly good Greek ones.[312] Furthermore, the alternative explanation, || that the phrase οἱ Βρέντεω παῖδες describes a phratry, seems almost certain to be correct, since there are good Greek parallels for such terminology.[313] There is no good reason, therefore, to find friendly natives here.

We have now looked at the evidence and arguments which have been adduced in favour of the theory that the Parian colonists established themselves peaceably (or made an alliance with the local inhabitants, met a friendly reception, etc.), and have found nothing which carries conviction. What of the evidence and arguments in the opposite sense, that they took Thasos by force?

As we have seen above, references in Archilochus and in Sosthenes' inscription certainly imply fighting between Parians/Thasians and

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309 *Nach. Gött. Gesells.* (Altertums) i (1934-61) 51. The theory was regarded as attractive by Pouilloux, *Et. Thas.* III 27.

310 See n. 228 above.

311 Treu in *RE* Suppl. XI, s.v. Archilochos, 136f. and L. H. Jeffery, *Archaic Greece* (London, 1976, 57; cf. Pouilloux, *BCH* lxxix (1955) 85.

312 Pouilloux. *op. cit.* 84f., for Thracian names and words, see also V. Georgiev, *La Langue Thrace* (Sofia, 1957) 60.

313 Apart from those adduced by Pouilloux, 83f., see the inscription from Selinus, published in *Kokalos* xvi (1970) 269: ho Μιλίχιος τᾶς πατριᾶς τᾶς h(ε)ρμίō παίδōν καὶ τᾶν Εὐκλέα παίδōν.

Thracians, and it is at least possible that not all this fighting was on the mainland.[314] It is wrong to try to explain away the clear implication of the phrase 'Thracian dogs',[315] and these Thracians appear to be on Thasos. We have also seen that the story that Heracles took the island by force from the Thracians and gave it to the grandsons of Minos, whom he had collected at Paros, seems unambiguously to mirror a forcible act of colonization on the part of the Parians.[316] Mythical stories were frequently used to justify the capture of land in Greek colonization.[317] It was in the light of this evidence that Bernard suggested, as we saw above,[318] that the destruction by fire, which he had discovered in his Sondage I, was evidence of the struggles on Thasos in the early days of the colony's existence. Such a suggestion can only be provisional when the area explored is so restricted. On the other hand, the literary evidence, even if small in quantity, and, in the case of Archilochus, difficult to interpret with certainty, does in general definitely point to hostility between the colonists and the native population. So, while there is no evidence and no good argument in favour of peaceable relations, there is at least some evidence of hostility.

It has been well observed that one of the most striking features of the Parian colonization of the area was its rapidity.[319] The excavations at Aliki have shown that there was here a rich archaic site occupied from the seventh century.[320] Since it is situated in the SE. corner of the island some 20 km. from the city of Thasos, it is reasonable to assume that the whole island was very quickly under the control of the Greek colonists. We are not dealing here, therefore,

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314 Pp. 84f. and nn. 234-7.

315 As Treu, *op. cit.* 137, who says that the poem is really an attack on Peisistratos, and the Thracians come better out of it than he does.

316 See above p. 82 and n. 253. The myth is well analysed by Jacobs, *Thasiaca* 8-10. He thinks that an original story about Alcaeus and Sthenelus, grandsons of Minos (and from Paros) was later inserted in the tale of Heracles' ninth labour, because of the importance of Heracles at Thasos.

317 For example, the various justifications used by the Athenians for their colonization of Sigeum (Aesch. *Eum.* 397ff., Hdt. v 94. 2), of Lemnos (Hdt. vi 137-40), of Scyros (Plut. *Cim.* viii 5f.), and of Amphipolis (Aeschines II. 31); but see also my *Colony and Mother City*, 204f. It was not only the Athenians. Consider, for example, Gela and Acragas; T. J. Dunbabin, *The Western Greeks*, 112, 318. Cf. in general the remarks of E. Sjöqvist, *Sicily and the Greeks* (Ann Arbor, 1973) 2f., 9, and, regarding Thasos, Birgitta Bergquist, *Herakles on Thasos*, 36.

318 P. 70.

319 Salviat, *VIIIe Cong. Arch. Class.* 299.

320 *Ibid.* and *BCH* lxxxvi (1962) 949-59, especially 957. *Guide de Thasos* 84-8.

with a small off-shore island, such as Berezan,[321] nor even with a situation, as for instance at Gela, where a Greek colony was able gradually to win control over a substantial territory.[322] The Parian colonists of Thasos succeeded in acquiring, in a single act of colonization, a territory which was in size fully comparable with Paros itself. The later Athenian capture and colonization of Lemnos[323] seems to offer a better analogy than the more distant colonial enterprises which we know of in the eighth and seventh centuries. ||

The rapidity of the Parian colonization of Thasos was matched by the speed with which they established themselves on the adjacent mainland. In addition to the evidence of Archilochus that, during his lifetime, they were disputing with Maronea over the possession of Stryme, we now have archaeological evidence to show that Neapolis and Oesyme were settled by the third quarter of the seventh century, and chance finds suggest a similar date for the establishment of the Thasian colony at Galepsus.[324] This successful colonization on the mainland so soon after the colonization of Thasos itself shows the power of the Parian colonists, and the attempt to win Stryme shows the range of their ambition.[325]

In view of what is known about the later prosperity of Thasos, and of what we may now confidently accept about Phoenician activity

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[321] A. Wasowicz, *Olbia pontique et son territoire* (Paris, 1975) 31-4; E. H. Minns, *Scythians and Greeks* (Cambridge, 1913) 451-3.

[322] See G. Vallet, *Atti sett. convegno. . . . Magna Grecia* (Naples, 1968) 98-100, and E. Sjöqvist, *op. cit.* 39ff.

[323] See my *Colony and Mother City*, 175ff.

[324] On Stryme, see above n. 237. The archaeological evidence for the early colonization of the Peraea is given by E. Leventopoulou-Giouri, *Arch. Delt.* (Chr.) xx (1965) 447ff.; especially 451: cf. also H. Koukouli-Chrysanthaki, *Arch. Delt* (Chr.) xxiv (1969) 349-51; *AAA* v (1972) 474-85; vi (1973) 230-40; D. Lazaridis. *Thasos and its Perasia* (Athens, 1971).

[325] Even if we accept, as we probably should, Bakalakis' identification of Stryme with a small peninsula some 7 km. *west* of Maronea (i.e. not east, as apparently implied by Hdt. vii 108f.); see G. Bakalakis, *Proanaskaphikes Erevnes sti Thraki* (Salonica, 1958) 91-7. Bakalakis' topography was accepted by Salviat, *VIIIe Cong. Arch. Class.* 299. It certainly makes it easier to understand how the Thasians could make Stryme part of their peraea. Thasian expansion even further east has been assumed on the basis of the story, preserved solely by Dion. Byz. (ed. R. Güngerich, Berlin, 1927) 19f. (48; cf. *GGM* II p. 37, frg. 30), that Archias of Thasos tried to settle the area called Archium (?) in the Bosporus, was repelled by the Chalcedonians, and then retired to Aenus. This story has been taken seriously, e.g. by C. Fredrich, *IG* XII, 8 p. 76, and J. Bérard, *L'expansion et la colonisation grecques*, 93f., but in default of better evidence and any indication of chronology little can be made of it by the historian.

in the region, we need not doubt that one of the motives, if not the main motive, of this great colonial venture was the desire to obtain mineral resources, notably precious metals. The results show that the Parian colonists had the resources, and we can easily understand that they had the motive, but another question remains. In view of the proximity of the area to Greece, and its very exceptional attractions, why does the Greek colonization come, relatively, so late?

There is, unfortunately, a complete lack of early foundation dates in the literary record for Greek colonies in the North Aegean area.[326] However, we know that Methone was settled about the time that the Corinthians established themselves at Corcyra,[327] and Archilochus' evidence shows that Torone and Maronea were colonized by his time.[328] It seems to me inherently probable that the main colonization of Chalcidice is early.[329] The first foundation dates in the area provided by the Greek chronographers are 655 for Acanthus and Stagira, and 654 for the first attempt to settle Abdera.[330] If we may trust Plutarch,[331] Sane was founded at the same time as Acanthus, which gives us three Andrian colonies in the same region founded at the same time. It is, therefore, a possible hypothesis that the fourth such colony, Argilus,[332] was also established at this time. Now that we have *c.* 650 or a little earlier as the probable foundation date of Thasos, the dates for the Andrian colonization in E. Chalcidice, and for the attempt of Timesias of Clazomenae at Abdera,[333] inevitably attract attention.

These dates have long been taken to imply competition among Greek states in the colonization of the Thracian coast.[334] Hostility

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[326] The foundation dates were usefully tabulated by R. M. Cook, *JHS* lxvi (1946) 77.

[327] Plut. *Q.G.* xi; see my discussion, *JHS* xci (1971) 46f.

[328] For Maronea, see Philochoros, *FGH* 328, fr. 43 = Harpocration, s.v. Στρύμη. The Τορωναί[ω]ν are mentioned in a fragmentary part of Mnesiepes' inscription (see above pp. 83ff.), B I 34. Although the interpretation of these lines is disputable, they clearly belonged to a long citation from Archilochus' works. For suggestions as to the interpretation, see Kondoleon, *Arch. Eph.* 1952, 88; Pouilloux, *Fond. Hardt* x. 18; Treu, *RE* Suppl. XI, s.v. Archilochos, 150f.

[329] See my remarks in *JHS* xci, *ibid.*

[330] All from Eusebius; see n. 326 above.

[331] *Q.G.* xxx.

[332] For this Andrian colonization, see J. Bérard, *L'expansion et la colonisation grecques*, 94, where the literary sources are cited.

[333] Hdt. i 168.

[334] As Bérard, *loc. cit.*

between Andrians and Parians is clearly reflected in Plutarch's story about the colonization of Sane.[335] Such a hypothesis seems even more likely to be correct with a lower date of the foundation of Thasos. Colonial competition has also been || suggested as the reason for the quarrels between Paros and Naxos in Archilochus' lifetime, but there is no supporting evidence.[336] Perhaps Torone is a more likely candidate. Although Archilochus' words about the people of Torone cannot be reconstructed with any assurance,[337] the fact that Heracles, in the ninth labour, after he has established the grandsons of Minos in Thasos, sails from there to Torone, where he kills Telegonus and Polygonus, perhaps suggests hostility between Thasos and Torone.[338] Such hostility is the more intelligible since we know that Chalcidians were involved in the Andrian colonization just mentioned.[339]

There is another conclusion which immediately confronts one when one considers these foundation dates, namely that at this particular moment there was opportunity for the Greeks to colonize in the Thracian area. It is as if some obstacle, which had previously kept them out of a region so close and so attractive, had suddenly been removed. The hostility and warlike skill of the Thracians has been seen as the obstacle in the way of Greek colonization in these parts,[340] but this hardly seems a sufficient explanation, for we should have to assume that, quite suddenly, several Greek states became strong enough to overcome the Thracian resistance. In any case, we know that this resistance was still strong enough to frustrate the colonization of Abdera,[341] just as it was, much later, to destroy attempts to settle Nine Ways.[342] Changes in Thrace or in Greece thus seem insufficient to explain why the Greeks should suddenly find this area open to colonization a little before the middle of the seventh century.

Is it too bold to suggest that the obstacle which was removed was the Phoenicians? We know of great disasters suffered by Tyre (and

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[335] See above n. 331.

[336] See Bérard, *loc. cit.*, who follows F. Lasserre, *Les épodes d'Archiloque* (Paris, 1950) 227f.; cf. Pouilloux, *Et. Thas.* III. 31.

[337] See above n. 328.

[338] Apoll. *Bibl.* II 5. 9. 14.

[339] Chalcidian participation is attested at Sane and Acanthus by Plutarch, *Q.G.* xxx; and at Stagirus by Dion. Hal. *Ad Ammaeum*, 5.

[340] Cf. my remarks in *JHS* xci (1971) 39, 46f.

[341] Hdt. i 168.

[342] Thuc. IV 102. 2-3.

Sidon) at the hands of the Assyrians in this very period. Esarhaddon destroyed Sidon in 677, and in 671 Tyre joined Egypt in an unsuccessful revolt which he crushed. In 668 Tyre was besieged by Assurbanipal.[343] Gjerstad suggested that the expansion of Kition *c.* 650 was the result of these disasters in Phoenicia proper, since Cyprus served as a place of refuge.[344]

Since Gjerstad wrote another, more important, consequence of the disasters to the Phoenicians in their homeland, and especially Tyre, was postulated by Forrer, namely the foundation of Carthage.[345] Forrer argued that Carthage, 'the new capital city' (N.B.), was founded to be the centre of a western empire to take the place of Tyre, which had been rendered powerless by the Assyrians. This bold and original suggestion has found favour because it seems to fit the archaeological finds in the western Mediterranean.[346] If the Phoenicians, who had been exploiting possibilities in the North Aegean, were diverted to the West, we could easily understand that they would leave a vacuum on the Thracian coast.

We know too little to press this suggestion, but disasters in Phoenicia at this time would seem in theory to offer the kind of explanation we seek for a sudden and general change in the Thracian region.[347] ||

It seems to me possible, therefore, that the island of Thasos and the coast of Thrace opposite were occupied before the Greek colonization by Thracians, who lived in a mutually beneficial relation-

[343] See S. Moscati, *World of the Phoenicians* (London, 1968) 20-2; Donald Harden, *The Phoenicians*, 49f.

[344] *Swedish Cyprus Expedition* IV 2. 462.

[345] E. Forrer, "Karthago wurde erst 673-663 v. Chr. gegründet," *Festschrift Franz Dornseiff* (Leipsig, 1953) 85-93.

[346] E. Frézouls, 'La fondation de Carthage,' *BCH* lxxix (1955) 53-176; E. Sjöqvist, *Sicily and the Greeks*, 15f.; F. Barreca and others, "L'espansione fenicia nel Mediterraneo," *Studi Semitici* xxxviii (1971) 124ff.; C. R. Whittaker, "The Western Phoenicians as colonisers." *Proc. Camb. Phil. Soc.* XX (1974) 78. Although the first settlement at Carthage cannot be put as late as the seventh century on archaeological arguments, there was a great access of population and increased evidence for foreign contacts in the middle of that century; see Frézouls, 166-8; Whittaker, 66. But for Forrer's thesis a refoundation, or transformation by reinforcement, of Carthage in the second quarter of the seventh century is just as suitable.

[347] A different explanation of a general sort for the Andrian colonization was proposed by Hammond, *History of Macedonia* I 440, who thought that the turmoil in the Chalcidic region created by refugees from Macedonia produced the opportunity for colonization. But, apart from the uncertain dating (see above n. 44) it is not immediately clear that an *influx* of peoples into the area would have opened it up for Greek colonization.

ship with Phoenicians. The latter not only provided wealth by their trading activities and exploitation of the local mineral resources, but were also able through their seamanship and military skills to protect the Thracian settlements.[348] The attractive area was thus closed to Greek colonization. The departure of the Phoenicians left the Thracian settlements, which were on or near the sea, vulnerable to the Greeks, who, like the Phoenicians, were at home on the sea and in battle. The Greeks of the Aegean, who were in the best position to see and exploit the new-opportunity, competed for control of the area, and the lion's share was won by Paros.

We may now revert to the question whether the Parian colonization of Thasos was peaceable or by force. If the above reconstruction has any validity, it will be apparent that the colonization of Thasos (and of the coast opposite) is not likely to have been truly peaceable. The Andrian (and Chalcidian) settlers are said to have seized existing cities; Sane was betrayed to them and Acanthus abandoned in fear by its native inhabitants.[349] The Parian colonization seems likely to have had similar characteristics, and, as we have seen, what evidence we have points in that direction.[350] If it is correct that the native settlement on Thasos at Kastri did not continue after the arrival of the Greek colonists,[351] it seems probable that here too the effects of their arrival on the native population were, to use the modern jargon, 'negative'.

It might be objected that the Thracians appear to have had good relations with the Phoenicians, so why should they not welcome the Greeks as another more advanced people offering a satisfactory symbiosis? It is possible that in time they did, on the mainland at least.[352] But the Greeks colonizers were very different from the Phoenicians, and much more demanding.[353] With their desire to control and occupy the land, they can hardly have been welcome to the Thracian inhabitants.

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[348] In their homeland the military strength of the Phoenicians at this period was considerable; cf. Moscati, *op. cit.* 19.

[349] Plut. *Q.G.* xxx.

[350] See above p. 94.

[351] See above p. 72.

[352] This is one reason why it is often said that relations between Thasians and Thracians were fundamentally good and peaceful; cf. Salviat. *VIIIe Cong. Arch. Class.* 303; Koukouli-Chrysanthaki, *AAA* vi (1973) 239.

[353] The contrast is well brought out by van Berchem, *Syria* xliv (1967) 74f.

Conclusion

We have ventured far into the regions of uncertain hypotheses which are perhaps unavoidable in the history of the seventh century. It is worth resuming, therefore, the conclusions of this enquiry which seem most definite and secure. In the second section we found that there is no literary evidence to put the date of the colonization of Thasos earlier than the *floruit* of Archilochus. This allows two further happy conclusions: (1) that the indications of archaeological and literary evidence about this date are now in perfect harmony, and (2) that the distinction between precolonial and colonial, which seems so clear in the archaeological record, need no longer be blurred in order to meet the supposed requirements of the literary evidence.

A further conclusion, important for the general history of Greek colonization, now also seems certain. The suggestion that Thasos should have been the first Greek colony in the area, because its site is the most attractive, and therefore that the colonization of Chalcidice should be later than that of Thasos, was perhaps always rather perverse, since, as we have seen, it is formally contradicted by some of our literary evidence, but it does now seem to be finally disproved by the chronology and character of the colonization of Thasos. We should surely see that ll colonization as almost the end, rather than the beginning, of Greek settlement in the North Aegean.[354] As we have seen, in its rapidity and scale it is not at all typical of the small expeditions of the early archaic colonizing movement. Rather it looks forward to the large-scale imperial colonization of later times.[355]

But it would be over bold to claim too much for the suggestions in this paper. In so far as it is based on very limited archaeological investigations it is bound to be overtaken by subsequent discoveries. It is offered, therefore, quite consciously as no more than a working hypothesis, which tries to explain in the most simple and probable fashion the evidence we now possess.

It is easy to think of topics which we may hope will be clarified by further archaeological work. First, perhaps, the extent and character of the settlement which preceded the Parian at Thasos. Then there is the question whether that settlement suffered violent destruc-

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354 Cf. my discussion, *JHS* xci (1971) 46f. Hammond, *History of Macedonia* I, 432 n. 2 and 440, still thinks of the Chalcidian colonization of Chalcidice as *c.* 650.

355 Cf. my *Colony and Mother City*, chs. III and IX.

tion as a whole. Closer chronological definition may also be achieved. There are also the questions raised by the striking comparisons between pre-Greek Thasos and the contemporary civilization on Lemnos. Have the enigmatic Pelasgians some part perhaps in the history of Thasos?

On these and many other questions future archaeological work may throw light. And it is earnestly to be hoped that archaeologists will bear them in mind. For while Thasos may as yet offer tantalizingly little archaeological evidence for the history of its foundation, this little has already shown that it is potentially an exceptionally informative and rewarding site. In the history of Greek colonization the nature and extent of precolonial occupation, and the relations of the Greek colonists to the native population, are vital topics. Progress in these questions largely depends on archaeological evidence, and archaeological evidence on precisely these topics has been shown to exist at Thasos. ||

11. ON THE GREAT LIST OF THEORI AT THASOS

We know of three inscribed lists of magistrates at Thasos: one of the archons and two of the theori.[1] In character they are comparable to the complete list that we possess of the stephanephori at Miletus, to the archon list at Athens, and to the many other similar lists that we know existed in ancient Greek cities.[2] The original position of the list of archons at Thasos is not known, nor that of the list of theori inscribed on white marble, but the great list of theori, of which we have very substantial remains, was originally on the inside of one, or possibly both, of the walls of the monumental passage called by the French scholars, to whom we owe so much of our knowledge of it, "le passage des théores."[3]

The destruction of this building, and the circumstances in which it and the inscriptions on it were first discovered in modern times,[4] have created great difficulties for those who have tried to reconstruct the inscription in its original state. For more than a century scholarly labours have been devoted to attempts to put the numerous extant parts of the inscription in their correct order, to establish the chronology and to interpret the constitutional implications of the document. The most recent of those attempts is by F. Salviat, who, in a characteristically learned and perceptive study, has made important advances and proposed stimulating new hypotheses.[5]

On the basis of his and his predecessors' discoveries several facts about the inscription may be regarded as firmly established. The building in which it was inscribed was erected in the first third of

[1] J. Pouilloux, *Recherches sur l'histoire et les cultes de Thasos* (hereafter *Rech.*) I (Paris 1954) chapter vi, "Les archives thasiennes," 238-286.

[2] M. Guarducci, *Epigrafia Greca.* II (Rome 1969) 328-416.

[3] G. Daux and others, *Guide de Thasos* (Paris 1967) 37-39. For an account of the excavations to which we owe our present knowledge of the building and its surroundings, see G. Roux, "Le 'passage' des théores et le problème du prytanée thasien," *BCH* lxxix (1955) 353-364. For a recent discussion of the building's function and meaning, see J. Pouilloux, "Une énigme thasienne: le passage des théores," *Thasiaca* (*BCH* Suppl. v, 1979) 129-141.

[4] For a full description, see Pouilloux, *Rech.* I 243-247.

[5] "Les colonnes initiales du catalogue des théores et les institutions thasiennes archaïques," *Thasiaca* (*BCH* Suppl. v, 1979) 107-127. In what follows I refer to this paper by the author's name alone.

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the fifth century, if we may date it by the style of the sculptured reliefs—Apollo received by the Nymphs and Hermes received by the Graces—which once adorned the west and east walls.[6] At least a century later, it was decided to inscribe in the building the names of the theori who had held office before that time, and thereafter to add the names of those who held office subsequently. After the large inscription listing earlier theori had been cut, the Thasians continued to add to it the names of subsequent theori down to the Roman imperial period, and the great list spread, so far as we can judge, over the whole of the west || wall.[7] My concern here is, however, solely with the first, recapitulatory, part of the inscription, of which, again, some features are established with certainty.

Over most of the list the names of the theori occur in threes (triads, colleges of three), which are carefully marked off by paragraphing lines in the first, recapitulatory, part of the inscription. These triads are introduced by the words: ὑπὸ τὸν χρόνον ὃν οἱ ἑξήκοντα καὶ τριηκόσιοι ἦρχον οἵδε ἐθεόρεον (*IG* xii.8.276). Thasian documents exist in which three theori are named besides the three archons, who were eponymous.[8] It has, therefore, long been accepted that, from the time when the regime was established, which could be described as the rule of the 360, the office of *theoros* was annual and there were three per year.

The dating of the earlier parts of the list of triads is in general terms very satisfactorily established by the college of Ἀντιφῶν Κριτοβούλου, Ἀθήνιππος Κλεολόχου and Κλέοχος Ἀλκίππου.[9] For this college presided over a decree (*IG* xii.8.263) confiscating the goods of certain citizens of Thasos and Neapolis, according to the decision of the 300, a decree that we can confidently associate with the famous Thasian decree encouraging delation (ML 83) and with the oligarchic regime which ruled at Thasos, at the longest, from 411 to 408/7.[10]

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[6] Salviat, 109-111; Pouilloux, "Une énigme thasienne etc.," 129-135.

[7] It is generally agreed that Miller's account of his discoveries shows that the list of theori was very largely, or totally, confined to the west wall. See Salviat, 111; Fredrich, *IG* xii.8 page 89.

[8] E.g. *IG* xii.8.267; xii. Suppl. 355, and on several others by restoration. Cf. Pouilloux, *Rech.* I 239.

[9] *IG* xii.8.277 D 81-3; Salviat's fold-out between pp. 116 and 117, col. 5, 20.

[10] See Pouilloux, *Rech.* I 145-146; cf. Tod, *GHI* II, Commentary to 98. The doubts expressed about this well-established historical reconstruction by M. B. Walbank, *Athenian proxenies of the fifth century* BC (Toronto, Sarasota 1978) 327-328, seem to me captious.

Antiphon, son of Critobulus, was actually one of the archons under whom the decree about delation was passed, and he is also mentioned in Hippocrates, *Epidemics* I.xv (Loeb). So we have here one college of theori which is closely dated to the period 411-408/7.

The position of this college within the whole list has been established by the patient labours of epigraphists, who, working from the inscribed blocks themselves, and from the prosopographical indications of names and patronymics, have succeeded in convincingly joining together a large group of blocks, which gives us parts of the first nine columns of the inscriptions.[11] It was recognized long since, largely on prosopographical grounds, that each column of the triads represented about a generation. On the basis of these data, it can be calculated within reasonably close limits that the 'rule of the 360' and the annual colleges of three theori began in the later years of the sixth century, that the change of writing preceptible in the seventh column,[12] which marks the end of the recapitulatory part of the list, occurred somewhat before the middle of the fourth century; and that the joining group of blocks takes us in the ninth column as far as the early part of the third century.[13] So the chronology of the list of triads for the first two centuries and more may be regarded as reasonably accurately established. ||

Before the triads began the list recorded a small number of theori under different headings. These irregular colleges, as they have been called, together with the beginning of the triads, all fell in the first column of the inscription. We know this because they are inscribed on the four blocks that we possess which certainly formed part of its left-hand edge.[14] It is Salviat's chief new contribution to change the order of these blocks.

This achievement forms part of his wider attempt to reconstruct the number and order of courses in the walls of the passage of theori. At the bottom of the wall he is able to show that there were, above the socle, four courses of blocks 0.465 m. high, some of which are still *in situ*.[15] Blocks of this height only contain late parts of the

[11] The grouping of Pouilloux, *Rech.* I, fold-out opp. 286 (see Salviat's fig. on p. 109) has been enlarged by the convincing attachment of *IG* xii.8.279 and 280. See Salviat, 112-114 and fig. 4 and 6.

[12] See Fredrich, *IG* xii.8. page 92 and Pouilloux, *Rech.* I 263.

[13] Pouilloux, *ibid.*

[14] See Salviat, 108; Pouilloux, *Rech.* I 284-285.

[15] Salviat, 109-111.

theori list.[16] The original list, which goes from the beginning to the third century BC, was inscribed on the higher courses, consisting of blocks of three different heights: 0.385 m., 0.31 m. and 0.295 m.[17]

The order in which courses of these differing heights were placed in the wall is established, firstly, by the block which is recognizably the beginning of the whole inscription, *IG* xii.8.273, bearing the words, in larger letters, Ἀγαθῆι Τύχη[ι]. This block manifestly occupied the top left hand corner; it is 0.295 m. high.[18] Another block of the same height (Thasos inv. 1313 = *Rech.* II no. 406),[19] which bears parts of two columns of triads and, at its upper-left hand edge, a *nu* of the same size and at exactly the same distance from the top edge as the letters of Ἀγαθῆι Τύχη[ι], has been convincingly placed in the same top course to give us, by restoration, the full first line, inscribed in larger letters: Ἀγαθῆι Τύχη[ι οἵδε ἐθεόρεο]ν.[20] So the two blocks of 0.295 m. that we possess clearly belong to the top course of the wall. Secondly, the large group of joining blocks that we have already mentioned establishes the fact that blocks 0.31 m. high stood above those of 0.385 m. For this group consists of three courses 0.385 m. high surmounted by two of 0.31 m.[21] These findings are sufficient to prove Salviat's contention that the wall was built of courses diminishing in height from bottom to top.

To find the number of courses in the wall is more difficult, and requires us to concentrate attention on the blocks of the left-hand edge (Fig. 8). These are: *IG* xii.8.273 (0.295 m.), 274 (0.31 m.), 275 (0.31 m.) and 276 (0.385 m.).[22] The first of these is the block in the top left-hand corner already mentioned. 276 has been proved to make a lateral join to the right with 277 A-B, part of the top course of 0.385 m. blocks in the big joining group.[23] 274 makes a good fit both physically and epigraphically directly || above 276 and 277 A-B.[24] Thus the remaining block, 275, since it is 0.31 m. high, must come between 273 and 274. This new order proposed by Salviat seems

[16] Fredrich, *IG* xii.8. page 89.
[17] Salviat, 111.
[18] Salviat, 108.
[19] C. Dunant and J. Pouilloux, *Rech.* II (Paris 1957) 231-233.
[20] Cf. Salviat, *ibid.*
[21] See Salviat's fig. on p. 109 as corrected by fig. 6.
[22] Salviat, 108.
[23] *Ibid.*
[24] See Salviat, 115 and fig. 5.

thoroughly convincing. If 275 is placed immediately beneath 273 and immediately above 274, we should have, definitely attested, six courses (Fig. 9) as follows: I 0.295 m., II 0.31 m., III 0.31 m., IV 0.385 m., V 0.385 m., VI 0.385 m.

However, Salviat (115) rejects the possibility that 275, which he places (Fig. 8) immediately beneath 273, was directly on the top of 274. Although he allows that the epigraphical criteria would be favourable for such a position, he thinks that it is excluded architecturally, because it would create a vertical alignment of joints, which is completely unacceptable in the monumental architecture of this building. We may agree with this last point, but still have doubts whether, in their original state, the right-hand edges of 275 and 274 would have been in vertical alignment. Neither of these blocks has its original right-hand edge preserved. The parts of our inscription which were brought from Thasos to the Louvre, which include both these blocks, were not the original stones, but merely the inscribed faces, which were sawn off for ease to transport.[25] The present right-hand edge of 275 was created by that operation. As for 274, its damaged right-hand edge is obvious from Salviat's photograph (fig. 5) and drawing (fig. 4).

We know from the parts of the lower courses which are still *in situ* that in some instances the vertical joints of two successive courses were not far apart, say *ca.* 0.20 m.[26] Thus, if the original edge of block 275 was quite a modest distance to the right of its present edge, a satisfactory relationship of vertical joints with 274 would have been created. True, Salviat says (115) that, in his opinion, very little of the block is missing at the right side, but this is surely difficult to prove. Even if it is right, there remains the other possibility, that the original right-hand edge of the lower block, 274, was further to the right than its present furthest point. This point is already about 0.08 m. to the right of the present edge of 275 (to judge by Salviat's fig. 6), so the extension we should need to postulate is not large.

[25] Cf. Salviat, 115. We can judge what a cruel handicap this circumstance has imposed on those who have essayed the reconstruction of the wall, by comparing the experience of the German archaeologists at Miletus, where the east wall of the Delphinion presented a similar task. There very small differences in measurements, carried out, as they were, on the original blocks, often provided the clue to their position. See A. Rehm and G. Kawerau, *Das Delphinion in Milet* (Berlin 1914) p. 170.

[26] See Roux, *BCH* lxxix (1955) 353-364, figs. 18, 19, 20.

These possibilities throw some doubt on Salviat's assumption that we should postulate a missing block, 0.31 m. high, at the left-hand edge of the inscription. This assumption, it should be noted, demands the restoration of three courses of 0.31 m. blocks throughout the inscription, and thus has important consequences for the reconstruction of the whole. In particular, it requires the postulate of another missing block in columns 3 and 4, between block *Rech.* II no. 406 (= Thasos inv. 1313) and *IG* xii.8.279.[27] Yet in both these places, in column 2 and in column 4, there are epigraphical indications which tell against the supposed *lacunae*.

The last two lines of column 2 on 275 contain the first two lines of a college of three, the beginning marked with the usual paragraphing line, while the first line of || this column in 274 contains the fragmentary remains of a name, under which another paragraphing line signals the beginning of a new triad in line 2:

275 col. 2, lines 11-12	Γόργος Ἐχεκράτ[ευς]
	Ὕψιτος Φανόλεω
274 col. 2, lines 1-2	Σ (--------)
	Γ[όργος Ἀρ]ιστομ[ήδου].

The last two lines of the second column (= col. 4) on *Rech.* II no. 406 similarly contain two names preceded by a paragraphing line. The first line of the first column (= col. 4) of 279 consists of a name succeeded by a paragraphing line:

Rech. II no. 406, col. 2, lines 7-8	Κλεόστρατος Ἀ[μφίεω]
	Χαιτίδης Ἀρι[στάρχου]
IG xii.8.279, col. 1, lines 1-2	Μέγων Πολυφάντου
	Ἱππαγόρης Νεστοπύριος.

There is thus a perfect epigraphical fit in column 2, if we place 275 directly above 274, and in column 4, if we place *Rech.* II no. 406 directly above 279. This means that in both places, if we reject this fit and postulate a *lacuna* of one block 0.31 m. high, we must also postulate that the missing blocks contained, in these columns, a number of lines divisible by three. For in this way alone would the same succession of triads from block to block be repeated. By my calculation, we have eleven columns of triads in blocks 0.31 m. high, where both the measurements and the numbers of lines are sufficiently

[27] See Salviat, fig. 6.

certain. Seven of these columns contain 12 lines,[28] two 11,[29] and two 13.[30] This shows that the requirements for Salviat's postulated missing blocks are certainly possible, since the most common number of lines is 12. Even so, it does not seem good method to me to reject the simple solution recommended by the epigraphical indications, in favour of a hypothesis based on a doubtful foundation (the supposed vertical alignment of the right-hand edges of blocks 275 and 274), which then requires the further assumption that, in two different places, there were a certain number of lines on the postulated missing blocks. We should conclude, therefore, that Salviat's hypothetical third course of blocks 0.31 m. high, while possible, is not probable, and we should dispense (Fig. 9) with this part of his reconstruction.

This decision has effects on the irregular colleges and on the calculations of the length of the columns, but, before we consider those effects, we must discuss a second course postulated by Salviat, for which there is no physical evidence. This is number VIII in his reconstruction, an additional course 0.385 m. high, situated || below the three definitely attested.[31] In postulating an extra course of this height, Salviat is following Pouilloux, who made a similar proposal in his great publication of 1954.[32] Pouilloux' argument was based, firstly, on the evidence for the lengths of the columns, as he then read it, and, secondly, on the belief that such an addition was "imposée par la succession des textes." The first of these bases has been superseded by new discoveries, and the precise meaning of the second was not spelt out. So it is not necessary to consider further Pouilloux' argument in favour of an extra course 0.385 m. high, but Salviat's is more specific.

This argument starts from the contention that there was a complete and constant number of colleges (triads) in each column. If that were so, the extant ending of column 5 in Salviat's course VII (*IG* xii.8.278 C) cannot be its original ending, since the last attested name on this block in this column is the first name of a new triad.

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[28] *IG* xii.8.275; 277 E, F, G, H; 279 col. 5; *IG* xii Suppl. 370.

[29] *IG* xii.8.274; 279 col. 4.

[30] *IG* xii.8.277 C, D. See Salviat's fold-out between pp. 116 and 117. The remains of column C are not enough in themselves to determine the number of lines, but Salviat's placing of 279 above this part of 277, and the succession of colleges that follows from that, require that there were 13 lines in column C.

[31] See Salviat, 115, 117.

[32] *Rech.* I 263.

Thus, it is argued, there must have been an inscribed block beneath it, which, for reasons of symmetry, must have been 0.385 m. high.[33] On this basis Salviat actually prints in column 5 a number of names in his postulated course VIII.[34] These names are taken from the second list of theori on white marble, and, as he himself admits (117), their presence at the bottom of column 5 in our list "n'est pas assurée en toute rigueure; ils pourraient se trouver également assisse I, en tête de la colonne 6."

However, the initial assumption that each column contained a complete number of colleges is open to doubt. Salviat's main reason for believing this lies in the comparison between the two lists of theori, that from the passage and that on white marble.[35] Where these lists overlap we find that the columns of the two lists do not line up, with the result that there is a displacement of the names ("décalage des noms"). Salviat claims that the length of this displacement is regularly two colleges, i.e. six lines, so that each column of the list on white marble seems longer, regularly, by two colleges. This shows, he contends, that both lists presented a constant number of colleges in each column.[36] This argument is complex and requires careful examination.

We possess three blocks of the list on white marble. The considerable overlaps between the names on these and the names in the list from the passage enabled Pouilloux to place the three blocks in relation to each other with complete certainty. When so placed they are found to contain parts of columns 4, 5 and 6 of the list in the passage.[37] This procedure also revealed the 'décalage' that we have mentioned. Where the list on white marble preserved more than one column, it was clear that the names in line from column to column were different from those in the same columns of the list in the passage.

There are two ways of measuring this 'décalage'. The one followed by Pouilloux was to take a name from, say, column 5 of the list on white marble, which we may || call name A, note the name (B) in line with it in column 6, and then check the name (C), which is line with name A in column 6 of the list in the passage. The

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33 See Salviat, 117.
34 See his fold-out between pp. 116 and 117.
35 See Salviat, 117.
36 *Ibid.*
37 See *Rech.* I no. 27 pp. 259-262, especially 260.

number of lines between names B and C in the list from the passage became his measurement of the 'décalage'. To take an example, Κάνωβος Κλεονίκου is the first well-preserved name of column 6 on the top block of the list on white marble (*Rech.* I no. 27 line 2). In line in column 5 is the name [Κρατιστόλεως Ἡγεκρ]άτεος, the small remains of which can be confidently supplemented from the list in the passage, with the help of calculations which are quite certain.[38] Thus Κρατιστόλεως Ἡγεκράτεος is name A in our scheme, and Κάνωβος Κλεονίκου is name B. Opposite Κρατιστόλεως Ἡγεκράτευς[39] in column 6 of the list in the passage we have Πυθίων Χοίρου,[40] who is name C. The difference between Κάνωβος Κλεονίκου (B) and Πυθίων Χοίρου (C) is five lines.[41] Thus Pouilloux regarded the 'décalage' between columns 5 and 6 as five lines.

Pouilloux also thought that he had found the same 'décalage' between columns 4 and 5, on the basis of the first line on the middle block of the list on white marble (*Rech.* I no. 27 line 20). Here we have the name Ἀντιφῶν Κριτοβούλου[42] in column 5. Nothing is preserved of column 4 on this block except for the letters -ος in the same line to the left of Ἀντιφῶν Κριτοβούλου. These clearly formed the end of a patronymic in the genitive from column 4. Pouilloux assumed that this must be part of the longest group of name and patronymic in the corresponding part of column 4, which is preserved in the list from the passage, and so restored [Ὀλυμπιόδωρος Κλεοφῶντ]ος. If that is right, and we call him A and Ἀντιφῶν Κριτοβούλου B, the name C from column 5 of the list in the passage is Ἡρόβουλος Νέβρου five lines above Ἀντιφῶν Κριτοβούλου.[43]

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[38] See *ibid.*

[39] The different spelling of the genitive ending of the patronymic is not a problem. By the fourth century εο and ευ were interchangeable in Ionic; see C. D. Buck, *The Greek Dialects* (Chicago 1955) 42.5 (p. 40). There are several examples of the same name spelt in both ways in the two lists of theori, e.g.: Δημάλκεος (*Rech.* no. 27 line 14), Δ[η]μάλκευς (*IG* xii.8.277.E.109); Κλεομένεος (*Rech.* I no. 27 line 34), [Κλ]εομένευς (*IG* xii.8.283.A10); Ἀσικράτεος (*Rech.* I no. 27 line 45), Ἀσικράτευς (*IG* xii.8.278.B.15).

[40] Κρατιστόλεως Ἡγεκράτευς is line 8 of *IG* xii.8.277.D, and Πυθίων Χοίρου is line 8 of 277.E, both columns being on the same block.

[41] Κάνωβος Κλεονίκου is line 13 of *IG* xii.8.277.E; see previous note.

[42] The patronymic is mistakenly printed as Κλεοβούλου on Pouilloux' fold-out, *Rech.* I opp. p. 260. But the correct spelling is perfectly clear on his plate XXI,1.

[43] See *IG* xii.8.277.C.49, D.77. The corresponding names in the two columns are not easy to discover from our publications, because the names in column 5 (*IG* xii.8.277.D) are closer together than those in column 4 (277.C). However, they are accurately presented by Salviat on his fold-out between pp. 116 and 117, and can

For our immediate purposes here it is necessary and sufficient to note that, even if these calculations showed a regular 'décalage' of five names in each column, a 'décalage' of that length would certainly not suggest that each column consisted of a complete number of colleges.

The second way of measuring the 'décalage', which is employed by Salviat, is not to use the physical facts on the stones of the list in the passage, but rather the succession of names and colleges. Thus he uses the physical facts on the list on white marble to obtain the correspondences between names A and B, but the || reconstructed succession of colleges on the list in the passage to measure the 'décalage'.[44] Following this procedure we find, to take the same instances as above, that Κρατιστόλεως Ἡγεκράτευς (A) is the first name of college 14 in Salviat's reconstruction of column 5 of the list in the passage, and Κάνωβος Κλεονίκου is the first name of what would all but certainly be college 16 in a similar reconstruction of column 6.[45] So the 'décalage' between columns 5 and 6, when measured in this way, comes out at six lines, or two colleges. Salviat also accepts Pouilloux' conclusion that the 'décalage' was the same in columns 4 and 5, which was based on the restoration, in the list on white marble, of [Ὀλυμπιόδωρος Κλεοφῶντ]ος in column 4, in line with Ἀντιφῶν Κριτοβούλου in column 5. Here Ὀλυμπιόδωρος Κλεοφῶντος (A) is the first name of college 18 in Salviat's reconstruction of column 4 in the list from the passage, while Ἀντιφῶν Κριτοβούλου (B) is the first name of college 20 in column 5. It is on this basis that Salviat concludes, firstly, that the 'décalage' was two colleges in length, and, secondly, that it was regular. It is not self-evidently good method to use the physical facts of the list on white marble, of which we possess rather little, as a bench-mark, and then to ignore the physical

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be checked with the help of the squeeze of the lines immediately below, of which a photograph is given in *IG* xii.8 page 98.

[44] The succession and numbering of colleges is pretty well certain in columns 4 and 5, where a large proportion is preserved; see Salviat's fold-out between pp. 116 and 117. If, as proposed above, we remove his postulated second course here, the uncertainties are even further reduced. (Note that, for the purpose of the present discussion, I keep to his numbering, in order to avoid confusion for those who consult his useful table). Of column 6, which Salviat does not include on his fold-out, there is more missing at the top, but even so, what we have, together with the clues given by the paragraphing lines, enables a close calculation to be made. For this column, see Pouilloux' fold-out, *Rech.* I opp. p. 262.

[45] See previous note.

facts of the list in the passage, of which we possess a lot, in measuring the 'décalage'. However, there is no need to press that objection. Let us make some more measurements of the 'décalage', following Salviat's procedure.

The best place to measure the 'décalage' between columns 4 and 5 is on the lowest block of the list on white marble. For here we are not forced to rely on one heavily restored name in column 4, but have the names from both columns 4 and 5 pretty well complete. Furthermore, the corresponding parts of the list in the passage are also quite well preserved on *IG* xii.8.285 and 278 B and C. On the list on white marble [Σκύλλος Φι]λίσ[κου] (A) of column 4 is in line with Λυσίστρατος Τηλ [εφάνεος] (B) of column 5 (*Rech.* I no. 27 line 40).[46] In the list from the passage Σχύλλος Φιλίσκου (A) is the last name of college 23[47] in Salviat's reconstruction of column 4, while Λυσίστρατος Τηλεφάνευς (B) is the last name of college 26 in his reconstruction of column 5. So here the 'décalage' is three colleges, or nine names. A 'décalage' between columns 4 and 5 of the same length is found throughout this block.

Such a large difference from the 'décalage' thought to obtain a mere 20 lines higher up in the same columns should make us look again at the basis for the smaller measurement, namely the restoration ['Ολυμπιόδωρος Κλεοφῶντ]ος. In fact, this restoration should have been rejected on other grounds. We know that the middle block of the list on white marble contained 20 names, or lines, in column 5.[48] In column || 4, therefore, the first name should be 20 lines above the first name of the same column on the next block, which, as we have seen, is Σκύλλος Φιλίσκου. On the duplicate list from the passage 'Ολυμπιόδωρος Κλεοφῶντος is 17 lines above Σκύλλος Φιλίσκου. The name which comes 20 lines above is Δεῖνις 'Ηγησιτέλους.[49] If this is the name which stood on the same line as 'Αντιφῶν Κριτοβούλου, it must have had the alternative spelling of the genitive ending, which we have already seen in Κρατιστόλεως 'Ηγεκράτεος. That is no difficulty, but the very much shorter group (17 letters long) conflicts with

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[46] The restoration of [Σκύλλος Φι]λίσ[κου] is made quite certain by comparison with *IG* xii.8.285.A.10 (with note).

[47] Mistakenly printed as 32 on Salviat's fold-out between pp. 116 and 117.

[48] See Pouilloux' fold-out, *Rech.* I opp. p. 260.

[49] See *IG* xii.8.277.C.46 and 49, with 285.A. The paragraphing lines prove that one name is missing at the top of 285.A; see Salviat's fold-out between pp. 116 and 117.

Pouilloux' assumption that the letters -ος should come from a particularly long combination of name and patronymic. However, that assumption rests on the belief that the spacing of letters was regular, which was certainly not the rule in the list from the passage.[50] That it was also not the rule in the list on white marble may be suggested by the name 15 lines below Ὀλυμπιόδωρος Κλεοφῶντος in the list from the passage, [Μ]ανδρόβουλος Ἀθηναγόρεω.[51] This has the same number of letters (22) as Ὀλυμπιόδωρος Κλεοφῶντος, and so might have been expected to extend onto the middle block of the white marble, if the length of the name were the sole factor involved. Even so, Δεῖνις Ἡγησιτέλους is a very short group, so it might be preferable to allow the number of names in each column to be unequal at this point, and assume that one of the names above and below Δεῖνις Ἡγησιτέλους stood in line with Ἀντιφῶν Κριτοβούλου. Both of these, Ἀνάξανδρος Σθένωνος and Ἀριστόμαχος Νύμφιος are one letter longer, and both have patronymics ending in -ος. This discussion shows that we should probably restrain ourselves from restoring the group ending in -ος in column 4 at line 20 of the list on white marble, but simple calculation shows that it can hardly have been [Ὀλυμπιόδωρος Κλεοφῶντ]ος.

For the purposes of our assessment of Salviat's argument from the 'décalage', it is relatively unimportant precisely what name stood there. We have already seen from the quite definite evidence lower down the list that, if we use his way of measurement, we find a 'décalage' of three colleges between columns 4 and 5, in contrast to the 'décalage' of two colleges between columns 5 and 6. Thus, whatever we may think of this method, it does not show us a regular difference in each column, and the argument that such a regular difference implies that each column had a constant number of colleges collapses. It is worth noting, finally, that even if we had a regular difference of six names, that would not absolutely require the assumption that each column consisted of a complete number of colleges.

The best argument in favour of a complete number of colleges in each column comes from the only block which preserves the tops

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[50] This is clearly visible on the photograph of a partial squeeze of the block *IG* xii.8.277.B-E; see page 98.

[51] *IG* xii.8.285.A.8. For the number of lines, see Salviat's fold-out between pp. 116 and 117 and above, n. 49.

of columns in the list of triads, *Rech.* II no. 406. For here each of the two columns represented (3 and 4) begins with a new college. This shows that columns 2 and 3 certainly ended with a complete college, and column 3 certainly consisted of a complete number of colleges. An argument against, however, appears to exist in the failure of the triads and their paragraphing lines to correspond from column to column, where we have blocks containing more || than one column (e.g. *IG* xii.8.277 B-E). Salviat's own answer to this apparent anomaly for his theory is to propose that the bottoms of the columns were not even;[52] but we have no example of such uneven ends of columns, so the theory is without positive support. Can it be helped by analogy? The great Law Code of Gortyn is a very large inscription, set out in columns on a wall, and here the column ends are not precisely even and in some cases overstep, if only slightly, the bottom of the blocks of the fourth course, which in general contains the lowest part of the inscription.[53] This very beautifully and carefully cut inscription is generally dated to the mid fifth century, that is a short hundred years before the Thasian document under discussion. On the other hand, the great inscription recording the admission of Cretans to the citizenship of Miletus, which was inscribed in the late third century in many columns over the east wall of the Delphinion, does end at the bottom of a course, according to the reconstruction proposed by Rehm, though the ends of the columns were not precisely even.[54]

So these arguments and analogies seem to be inconclusive. They may have been an extra course 0.385 m. high, as Salviat proposes, even though there is no physical evidence for it, nor any decisive argument in its favour. If there was, we cannot tell whether or not it was completely inscribed, or, as at Gortyn, merely received a line or so extra at the bottom of some columns.

If we rigidly confine ourselves to what is definitely attested, we are left with the six inscribed courses given above (p. 106), making a total height for the wall of 3.93 m., which is a significant reduction from that proposed by Salviat (4.625 m.) and much closer to

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[52] Salviat, 117.

[53] *IC* iv 72; cf. Guarducci's comments on p. 124 and the fold-out drawing opp. p. 142. See also R. F. Willetts, *The law code of Gortyn* (*Kadmos* Suppl. I, Berlin 1967) 3-4.

[54] A. Rehm and G. Kawerau, *Das Delphinion in Milet* (Berlin 1914) no. 33-38. See the section "Der Aufbau der Hallenwand," pp. 166-172.

the heights suggested by earlier investigators, as the 3.72 m. of Avezou and Picard,[55] or the 'close to 4 m.' of Roux.[56] It is also less distant from the only substantial figure that we possess for the height of the wall, which does not depend on calculation, namely 3.35 m., the height of one of the four monolithic pillars which originally formed the ends of the walls in question.[57] Miller found all four of these pillars, but only recorded the dimensions of one of them, without any indication whether that one was complete or not. Since they are long since all lost, these pillars give us no more than a minimum height of 3.35 m. for the walls, but it is desirable, in principle, to postulate as little as possible in addition.

It might seem tempting to recalculate the chronology of the triads on the basis II of six inscribed courses, as Salviat recalculated it for his higher reconstruction.[58] Thus, if one worked from this minimum, we should have from 24 to 26 plus colleges per column.[59] Before Antiphon's college there would be about 103 colleges. Putting his tenure, with Salviat, in 409, we should have the change of government and start of the triads in *ca.* 512 BC, and a date for the first inscribing of the list some what earlier than Pouilloux' *ca.* 360. Clearly a date of *ca.* 512 for the installation of the regime of the 360 would invite a number of interesting historical hypotheses, but, since I regard the question of a possible fourth course of 0.385 m. blocks, if nothing else, as undecided, such hypothesizing seems of doubtful value. So let us leave these calculations on one side in the knowledge that we cannot be sure of their precise truth, but can be confident that we have an approximately, and sufficiently, correct picture.

More interesting historical questions concern the irregular colleges at the beginning of the list, which have always presented greater problems of interpretation than the triads, and which are affected

55 "Les fouilles de Thasos," *CRAI* 1914, 276-305; see p. 293.

56 *BCH* lxxix (1955) 354.

57 See E. Miller, "Inscriptions inédites de Thasos," *Journal des Savants* 1872, 49-60, especially 59f.; cf. *Idem, Le Mont Athos, Vatopédi, l'île de Thasos* (Paris 1889) 393f. The matter was well discussed by Fredrich, 'Thasos', *Ath. Mitt.* xxxiii (1908) 215-246, especially 230f. For modern investigations, see Roux, *BCH* lxxix (1955) 254. The inscriptions on three of these pillars are published as *IG* xii.8.353, 354, 355.

58 Salviat, 118.

59 About 25 years seems a low figure for a generation in ancient Greece, but we should bear in mind the possibility that a prosperous colonial élite might contract early marriages.

by Salviat's new order and by our rejection of his postulated third course of blocks 0.31 m. high. We now have the following four stages at the beginning of the list:

1. (After the heading, Ἀγαθῆι Τύχη[ι οἵδε ἐθεόρεο]ν) ἐπὶ τῆς πρώτης ἀπαρχῆς, followed by twelve names, below the last of which there is a paragraphing line.
2. ἐπὶ τῶν δυώδεκα ἀρχόντων οἵδε ἐθεόρεον, followed by five names, below the last of which there is a paragraphing line.
3. ἐπὶ τῆς δευτέρης ἀπαρχῆς οἵδε ἐθεόρεον, followed by twelve names, below the last of which there is a paragraphing line.
4. ὑπὸ τὸν χρόνον ὃν οἱ ἑξήκοντα καὶ τριηχόσιοι ἦρχον οἵδε ἐθεόρεον, followed by the triads.

The novelty of Salviat's re-ordering is that the first and second *aparchai* are now separated by the twelve archons, and there are now twelve theori (instead of eight) under the first *aparche*. Our rejection of Salviat's postulated block 0.31 m. high reduces the names under the twelve archons from his seventeen (or sixteen) to five.

The almost complete absence of literary evidence on the constitutional history of Thasos[60] hampers interpretation, and we are inevitably thrown back on hypothesis. The chief difficulty is the meaning of ἀπαρχή. Since the twelve archons and the 360 are clearly governments or constitutions, Jacobs argued that ἀπαρχή cannot have its normal sense of 'first-fruits' or 'offering', but must be equivalent to ἀρχή, rule.[61] Although there is no analogy for such a meaning, he cited two appearances of ἄπαρχος in Aeschylus (*Agam.* 1227 and *Pers.* 327) in support of his interpretation. The reading in both Aeschylean passages is perhaps not entirely secure, but, if the Greek word is right, its general meaning as 'commander' or 'ruler' is not in doubt. Jacobs' interpretation of ἀπαρχή here was accepted by the editors of Liddell and Scott's lexicon, who give the meaning 'board of officials'. ||

Salviat has followed the same line, but refined it by adding the analogy of Pindar, *Nem.* iv.46, where another doubtful reading, ἀπάρχει, was rendered 'reigns afar' by Sandys in the Loeb. Following this way of understanding the word, and adducing the transitional arrangements for the new colony proposed by Plato in the *Laws*

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[60] Plutarch's information that a Thasian tyrant called Symmachos was expelled by the Spartans (*Mor.* 859 D) is discussed by Salviat, 123-125.

[61] E. Jacobs, *Thasiaca* (Berlin 1893) 15.

(751-755), Salviat suggests that ἀπαρχή here means a detached or local political power in relation to that of the mother city. He thus sees the college of twelve theori on each occasion as having a long term of office and the duty of putting in order the institutions of a newly-founded city, comparing them with Plato's *nomophylakes* in the *Laws*, with the ten people who took part in the foundation of Thurii and the ten ἀπ[οικισταί] of Brea. It follows, since we have two such colleges, that there were probably 'deux poussées colonisatrices' and 'deux actes de fondation'.[62]

This is a rather large historical hypothesis to be supported by the weak and uncertain foundations of the enigmas at the beginning of the list of theori, especially as some of the arguments in its favour are open to objection. Consider first the use of Plato's *Laws*. The true analogy in the *Laws* to the ten who took part in the foundation of Thurii and the ten ἀπ[οικισταί] at Brea is surely the board of ten who are charged with the foundation of the new colony.[63] And that board, it is clear, ceases to function when the life of the colony begins, because Plato makes all its members join the first *nomophylakes*, the 37 guardians of the constitution to whom he entrusts the difficult task of seeing that the new city obeys its laws in its early days and becomes accustomed to them.[64]

However, even if we remove the analogies from Thurii and Brea, what of the comparison with Plato's *nomophylakes*? It has been cleverly observed[65] that this section of the *Laws* combines two systems for launching the new city which are not compatible. The second system in our text requires the Cnossians to choose 200 commissioners: 100 Cnossians and not less than 100 from the oldest and best of the colonists.[66] These commissioners are to ensure that the magistrates be appointed according to the laws and undergo the δοκιμασία. After this the Cnossians go home. The first system in our text requires the Cnossians themselves to appoint the first *nomophylakes*, 18 from the Cnossians (including Cleinias and the other members of the board of ten founders) and 19 from the colonists.[67]

The reason why these two systems cannot be compatible is that

[62] Salviat, 120-121.
[63] *Laws* 702 C; 751 E.
[64] *Laws* 752 E-753 A.
[65] See Glenn R. Morrow, *Plato's Cretan City* (Princeton 1960) 204-205, 238-240.
[66] *Laws* 754 C-D.
[67] *Laws* 752 E-753 A.

the second system in our text arranges what must be the first election of *nomophylakes*,[68] since on Plato's system, whereby each individual *nomophylax* retires at 70,[69] it will only be on the first occasion that all the *nomophylakes* are elected at once. The first system, the appointment of the *nomophylakes* by the Cnossians, is thus not a preliminary but an alternative, and (unless Plato himself was guilty of inconsistency) there is much in favour of the theory that he had changed his mind, but both systems were found in || his papers and put in the text by his editor.[70]

The relevance of these discoveries to our problems lies in the fact that a historical parallel can be found for the system whereby citizens of the mother city formed half a commission to conduct the first elections and then returned home. As has been noticed,[71] this has similarities to the choice of the first βουλή at the reorganized Erythrae by the Athenian ἐπίσκοποι and φρούραρχος (ML 40 13-14). But this is not the system which Salviat uses as a parallel for his idea of the first and second *aparchai*. The *nomophylakes* appointed by the Cnossians are that, and Plato makes very clear by his writing that he is there proposing something quite unusual and hypothetical. Compare the phrases τοὺς Κνωσίους χρῆναι . . . μὴ μόνον ἀφοσιώσασθαι περὶ τῆς χώρας ἣ νῦν κατοικίζεται (752 D; "ought not to restrict themselves to bare ceremonial relations with the *land*"),[72] εἴπερ τινὶ τρόπῳ καὶ μηχανῇ γίγνοιτο ὀρθῶς (752 C), and the word πως before τοσοῦτον χρόνον (*ibid.*) with England's note.[73] In these circumstances we cannot admit this system as an analogy for arrangements in historical colonial foundations. Incidentally, also, it is wrong of Salviat to say that the *nomophylakes* served for 20 years, since, on Plato's system, that would only be true of those individuals who happened to be appointed at 50, the minimum age.[74]

So we have no historical analogy for the conception of a body of magistrates appointed by the mother city to serve a long term

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68 *Laws* 753 B-D.

69 *Laws* 755 A.

70 See Morrow, *ibid.*, who proposes to see the work of the editor, rather than Plato, in the linking sentence, προελθόντος δὲ χρόνου καὶ μεινάσης τῆς πολιτείας αἵρεσις αὐτῶν ἔστω τοιάδε τις (753 B).

71 Morrow, *ibid.*

72 England, note *ad loc.* (including italics), though he reads ἣν νῦν κατοικίζετε. See *The Laws of Plato*, edited by E. B. England (Manchester 1921) I p. 547.

73 *Ibid.*, note to 752 C6.

74 *Laws* 755 A-B.

supervising the new colony. But there is an even stronger objection to the interpretation advanced by Salviat, and that is the unusual sense of ἀπαρχή on which the whole theory depends. Some rather doubtful analogies of cognate words in Pindar and Aeschylus are surely not sufficient to persuade us that the common Greek word ἀπαρχή, with its well-established meaning, could stand, in a prosaic official document, for something quite different. We only have to imagine what we should think of a proposal that the English word 'tithe' could mean some form of government.

Ἀπαρχή is a common enough word in Greek inscriptions.[75] It appears from the archaic period to the Roman epoch, and its meaning develops from 'first fruits' to 'offering' in general. Although the greatest number of occurrences of the word comes from Athens,[76] a search through the available indices of our *corpora* reveals its use in many other parts of the Greek world.[77] The noun does not appear in extant || Thasian inscriptions, apart from the list of theori, but significantly enough, the verb (ἀπάρχεσθαι) does. In the law regulating offerings to Theogenes, which is approximately dated on the basis of the letter forms to the first century BC, the preliminary offering required (at least one obol) is expressed by the term ἀπάρχεσθαι.[78] It is interesting to observe that, in his discussion of the monument, Chamoux actually states that the law "institue l'obligation d'une ἀπαρχή."[79] This is enough to show that, as we could conclude on general grounds, the normal sense of ἀπαρχή was recognized at Thasos.

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[75] H. Beer, *Ἀπαρχή und verwandte Ausdrücke in den griechischen Weihinschriften* (Würzburg 1914), collected all instances known to him on pp. 8-9. More have been published since.

[76] As Beer noted, *ibid.*

[77] E.g. Boeotia, Ptoion sanctuary (*IG* vii.4139, 4146); Rhodes (*IG* xii.1.774, 775, 776); Samothrace (*IG* xii.8.156B.21); Magnesia on the Maeander (*I von M.*30.1; 31.35, decree of Acarnania, = *IG* ix.1.2.^{2} 582; 36.19f., decree of Ithaca; 47.26, decree of Chalcis; 53.40, decree of Clazomenae (?); 59.a.6, decree of Laodicea on the Lykos; 60.24; 82.10f.; 83.12f., decree of a Pergamene city; 85.22 and 27, decree of Tralles; 87.20, decree of a Pergamene city); Priene (*I von P.* 5.11); Ilium (*Inschr. griech. Städte aus Kleinasien, Ilion,* 10.39). It should be remembered that the existence and the quality of indices in our epigraphical publications determine the examples I have found.

[78] τοὺς θύοντας τῶι Θεογένηι . . . ἀπάρχεσθαι εἰς θησαυρὸν μὴ ἔλασσον ὀβολοῦ. See *Rech.* II n. 379; F. Sokolowski, *Lois sacrées des cités grecques* Suppl. 72; F. Chamoux, "Le monument 'de Théogénès': autel ou statue?," *Thasiaca* (*BCH* Suppl. v, 1979) 143-153.

[79] Chamoux, 150.

There is also the fact that theori have in general a specially close association with ἀπαρχαί in the normal sense of first fruits or offerings.[80] It is true that we do not know the functions of the theori at Thasos. The office is assumed, no doubt rightly, to have its roots in the theori of Paros,[81] who are found in one document investigating sacrilegious behaviour.[82] The placing of the list in a building of sacred character may well also be significant, though the association of the *list* with the building is strictly secondary, since the building existed for a long time before it housed the list.[83] Neither these indications, nor analogies of theori in other Greek states,[84] can tell us for certain the character and functions of the Thasian theori, but on general considerations it remains true that the association of theori with offerings would be something entirely normal and to be expected.

For the reasons stated Salviat's hypothesis about the meaning of the first and second *aparchai* seems to me unconvincing. I offer now a very different explanation, which, though inevitably just as hypothetical, is more in accordance with the evidence we have, if that evidence is allowed its natural meaning.

There are so few names in the irregular colleges, a mere 29, that it is clear either that the records of those who held office in the period between the foundation of the colony (*ca.* 650)[85] and the institution of the regime of the 360 were incomplete, or that there was not then a regular magistracy. There is one formal argument to support the idea that the theori of the irregular colleges were appointed for special occasions. The paragraphing lines which are used later in the inscription to mark off each annual college of three, appear

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80 For one good example compare the provisions of the decree of Priene, which is dated by the editors to shortly before 326/5 BC:

αἱρεῖσθαι δὲ τ[ὸν δῆ]μον θεωρο[ὺς] δύο τοὺς τὰς ἀπαρχὰς ἀπ[ο]ί[σοντας καὶ τ] ὰ ἱερὰ ποιήσοντας τῆι Ἀθηνᾶι τῆι Πολιάδι κα[θ'ἑκάστην] πεντετηρίδα, τοὺς δὲ νῦν αἱρεθέντας με[τὰ τῶν] ἱερῶν καὶ τὸ φήφισμα φέρειν καὶ ἀποδοῦν[αι τῆι] βουλῆι καὶ τῶι δήμωι τῶι Ἀθηναίων κτλ. (*I von P.* 5.10-15; dotted letters not noted).

81 E.g. by Fredrich, *IG* xii.8. page 89.

82 *IG* xii.5.108. They are restored in line 5 of 109, but without much justification. See Pouilloux, *Rech.* I 189, who does not adopt the suggestion in his new text of the document. Many suggestions have been made for the missing end of line 5; see Pouilloux' *ap. crit., ibid.*

83 As Roux, *BCH* lxxix (1955) 359. For the abundant evidence for the sacred associations of the building, see Pouilloux, "Une énigme thasienne: le passage des théores," *Thasiaca* (*BCH* Suppl. v, 1979) 129-141.

84 See *RE* s.v. theoroi.

85 A. J. Graham, "The foundation of Thasos," *BSA* lxxiii (1978) 61-101.

in these exceptional lists exactly three || times, and mark the end of each group of names under the three different headings.[86] This suggests that each group was a single college, rather than a list of successive office-holders over a considerable period of time.

We could thus translate ἐπὶ τῆς πρώτης (or δευτέρης) ἀπαρχῆς as (the following were theori) at the time of the first (or second) offering of first fruits, and assume that a college of twelve theori was twice specially appointed to make a special *aparche*. If we are tempted to conjecture what kind of special *aparche* this might be, we are inevitably reminded of the famous *aparche* of Siphnos, another island made rich by mining precious metals.[87] Such *aparchai* are easily imagined at Thasos, where suitable occasions for a special offering might well have been provided by the discovery, or by winning control, of a new mine or seam.[88]

The theori (now five) recorded as "in the time of the twelve archons" offer little scope for conjecture, as Salviat observes.[89] However, as Pouilloux noted,[90] the last name, Ἄναξις Χοίρωνος, invites comparison with Ἄνδρων Χοίρωνος, who figures in the first college of three, and Ἵππων Χοίρωνος, who appears in the second (*IG* xii.8.276.10 and 13). These two are manifestly brothers. If Ἄναξις is a third brother (the simplest hypothesis), then the theori under the twelve archons are approximately contemporary with the beginning of the triads. Another pointer in the same direction may be found in the likely connection between Κλειτώνυμος Νικίδεω, another *theoros* under the twelve archons (*IG* xii.275.A.9), and Πυθώνυμο[ς Νικίδ]εω of *IG* xii.277.B.26. In the latter name rather a large part of the patronymic needs to be restored, but the form of the name Πυθώνυμος is so similar to Κλειτώνυμος that the supplement is very attractive, and it is no surprise that recent editors print name and patronymic without any indication that they are partly restored.[91] Πυθώνυμος Νικίδεω was

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[86] Another paragraphing line is printed after line 9 in *IG* xii.8.273 (first *aparche*). However, it was questioned by Fredrich in his note, and is rightly omitted by Salviat in his fold-out between pp. 116 and 117. See the photograph of this stone in *Rech.* I, plate XXVI.

[87] See Hdt. iii.57.2. Herodotus' narrative points to a date in the third quarter of the sixth century; see How and Wells, commentary *ad loc.*

[88] See especially Hdt. vi.46.2-47.2. For a recent discussion of this passage, and a useful bibliography on the subject of Thasian mines, see B. Holzmann, "Des mines d'or à Thasos?" *Thasiaca* (*BCH* Suppl. v, 1979) 345-349.

[89] Salviat, 121.

[90] *Rech.* I 264.

[91] See Salviat's fold-out between pp. 116 and 117, column 3 college 19; Pouilloux, *Rech.* I fold-out opp. p. 262, column 3 line 25.

theoros about two generations after the triads began, so, if Κλειτώνυμος Νικίδεω was his grandfather, the theori under the twelve archons would again be shown to be contemporary with the institution of the regime of the 360. This chronology is all the more interesting after Salviat's re-ordering of the irregular colleges, since it must mean that the second *aparche*, which we now know came after the twelve archons, is also contemporary with the beginning of the triads.

That conclusion was already suggested by the prosopographical connections which Pouilloux noted[92] between the theori of these two lists and the list of archons. The name and patronymic Καλλιμήδης Θράσυος, one of the theori at the time II of the second *aparche* (*IG* xii.8.274.12), also appear in the list of archons (*Rech.* I no. 31 col. 2 line 3). Ὀρθαγόρης Χαρίλλου, one of the theori under the twelve archons (*IG* xii.8.265.7), may well be related to the Ἄναξις Ὀρθαγόρεω of the archon list (*Rech.* I no. 31 col. 2 line 7), and/or to the Ὀρθαγόρης of the archon list (*Rech.* I no. 28 col. 1 line 14). On Pouilloux' dating Καλλιμήδης Θράσυος and Ἄναξις Ὀρθαγόρεω were archons in the period *c.* 520-490, and Ὀρθαγόρης was archon in the period *c.* 490-460.[93]

The order and dating of the stones containing the archon list proposed by Pouilloux[94] may be open to question in detail, but there are sufficient prosopographical connections within the Thasian archives to give us confidence that they are approximately correct. If the archon Καλλιμήδης Θράσυος is the same man as the *theoros* of the same name, and if the archon Ἄναξ Ὀρθαγόρεω is the son, and the archon Ὀρθαγόρης the grandson of the *theoros* Ὀρθαγόρης Χαρίλλου, which are all reasonable assumptions, then the chronology already suggested by the sons of Choiron and by Κλειτώνυμος and Πυθώνυμος Νικίδεω is confirmed, and we may confidently place the theori under the twelve archons and those of the time of the second *aparche* approximately in the later years of the sixth century. These two irregular colleges seem, therefore, to have been single groups which shortly preceded the establishment of the regime of the 360 and the regular, annual, colleges of three theori.[95]

[92] *Rech.* I 264.
[93] *Rech.* I 263-278 and, for a tabular summary of his results, p. 276.
[94] *Ibid.*
[95] These prosopographical connections were noticed by Pouilloux, *Rech.* I 283f., who concluded that at least some of the extraordinary lists cannot be too far separated from what follows, and should be placed within the sixth century.

This chronology effectively excludes the possibility that the second *aparche* could have anything to do with a supposed second foundation of Thasos by Paros, and thus constitutes a further powerful obstacle to the meaning of ἀπαρχή proposed by Salviat. It also makes the hypothesis worth consideration that the twelve archons were themselves a short-lived administration, like the ten archons at Athens who completed Damasias' third year.[96]

The approximate chronological position of two of the irregular colleges has been seen to be satisfactorily established by prosopographical indications. What of the theori at the time of the first *aparche*? Here we now have twelve names and eleven patronymics, making twenty three personal names in all. Of these ten occur only here in Thasian prosopography.[97] Of the rest, eight are well attested,[98] and five are positively common.[99] This evidence shows clearly that, in general terms, the theori of the first irregular college are in no way isolated from their successors. In his discussion of the four inscriptions of the first column (*IG* xii.8.273-6) Fredrich rather strangely remarked that the names on them are scarcely found in other inscriptions.[100] This is far from the truth, as we have seen, and it certainly does not apply to the college at the time of the first *aparche*, a majority of whom seem to have || been members of families distinguished at Thasos over many centuries.

So much for one general conclusion from the prosopography of the first college. There are also chronological indications in the prosopographical data, though they are suggestive rather than compelling. Ἡγίων Ἀγασικλέος could be connected, perhaps as uncle, with Ἀγασικλῆς Λάβρου, one of the theori at the time of the second *aparche* (*IG* xii.8.274.6), and, possibly, also with Χαριδαντίδης Ἀγασικλέος, who was a *theoros* within the second quarter of the fifth century (*IG* xii.8.280.B.20).[101] Οἴνιχος Καλλίνου was probably related, perhaps as uncle, to Καλλίνους Ξενοδόκου, also a member of the college at the

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[96] Arist. *Ath. Pol.* 13.2. I owe this suggestion to Mr. T. J. Cadoux, who kindly discussed the list of theori with me.

[97] Μετήριτος, Κύδιππος, Εἰδομένης, Κύλων, Λέωμις, Ἐπίχριτος, Τεισίμαχος Ἡγίων, Σημαγόρης and Οἴνιχος. I rely on the *Prosopographie Thasienne*, Annexe IX, in *Rech.* II.

[98] Ἑρμόζυγος, Ξενοκλῆς, Ἀλκιάδης, Τιμαίνετος, Φόρυλλος, Ἐπικλῆς, Ἀγασικλῆς, Καλλίνους.

[99] Τηλεφάνης, Κάδμος, Ἀντίοχος, Νικήνωρ, Ἀριστίων.

[100] *IG* xii.8 page 92.

[101] Χαριδαντίδης Ἀγασικλέος is attested on a block of 0.385 m., which is now

time of the second *aparche* (*IG* xii.8.274.8). Since Καλλίνους is not a very common name at Thasos, it seems likely that there was also some relationship to the archon, Ῥέων Καλλίνου.[102] Evidence for the date of the archonship of Ῥέων Καλλίνου is provided by another name on the same stone, Χάριλλος Ἀριζήλου, for he is almost certainly connected with the *theoros*, Ἀρίζηλος Χαρίλλου (*IG* xii.8.277.A.9), who held office about a generation after the regime of the 360 was established.[103] If Χάριλλος Ἀριζήλου was the father of the *theoros*, the archonship of Ῥέων Καλλίνου is approximately contemporary with the beginning of the triads; if Χάριλλος Ἀριζήλου was the son of the *theoros*, Ῥέων Καλλίνου was archon towards the middle of the fifth century. Two of these possible relationships (Ἡγίων Ἀγασικλέος with Ἀγασικλῆς Λάβρου, and Οἴνιχος Καλλίνου with Καλλίνους Ξενοδόκου) would point to a gap of about a generation between the theori of the first and second *aparchai*. If that were so, the archon, Ῥέων Καλλίνου might be a younger brother of the *theoros* Οἴνιχος Καλλίνου, or, alternatively, grandson or grand-nephew, with long generations.

Since we lack matching pairs of names, linked as name or patronymic, we cannot press the argument that the first *aparche* occurred one generation before the second, though it is shown to be a possibility. At the least, the correspondences we have make it improbable that the first *aparche* was separated by a great distance in time from the rest of the list. Here too then, chronology is itself enough to rule out any connection between the first *aparche* and the original colonization of Thasos by Paros.

The indications that we have noted make it likely that all three irregular colleges and the institution of the regime of the 360 belong, in broad terms, to the second half of the sixth century.[104] Here again it might seem tempting to use this chronology as a basis for hypothetical reconstructions of the political and constitutional history of Thasos at this time. However, the virtual absence of literary evidence,[105] together with some uncertainty about the date of the first college,

placed beneath 277 AB; see Salviat's fig. 4. Thus he belongs in Salviat's scheme to column 3, course VI, college 21 (see his fold-out between pp. 116 and 117), and will have held office about two generations after the beginning of the triads.

[102] *IG* xii.8.403, but republished by Pouilloux, with important commentary, as *Rech.* I no. 33, pp. 271f.

[103] See Salviat's fold-out between pp. 116 and 117, column 2 college 18.

[104] Compare Pouilloux' view; see above, n. 95.

[105] See n. 60 above.

create so many unknowns, that any such hypothesis would be subject to inevitable doubts. Some of these doubts may perhaps one day be dispelled by further fortunate discoveries in || the rich mine of Thasian epigraphy. In addition, we may perhaps one day definitely know the number of courses in the walls of the 'passage des théores'. For the moment, however, we must emphasize the negative conclusion to which our arguments have led. Much as one might welcome[106] new evidence that Paros founded Thasos twice and was closely involved in the early affairs of the colony on both occasions, such evidence does not exist in the early part of the list of theori at Thasos.[107] ||

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[106] Salviat's hypotheses have been enthusiastically welcomed by R. Martin, "Relations entre métropoles et colonies: aspects institutionnels," *Θιλίας Χάριν. Miscellanea di studi classici in onore di Eugenio Manni* (Rome 1979) IV 1435-1445.

[107] My thanks are due to Mr. A. G. Woodhead, who kindly read this paper in draft and made several helpful comments and criticisms. I am grateful too to Dr. J. J. Coulton for valuable advice on architectural questions. Naturally they bear no responsibility for any statement I have made.

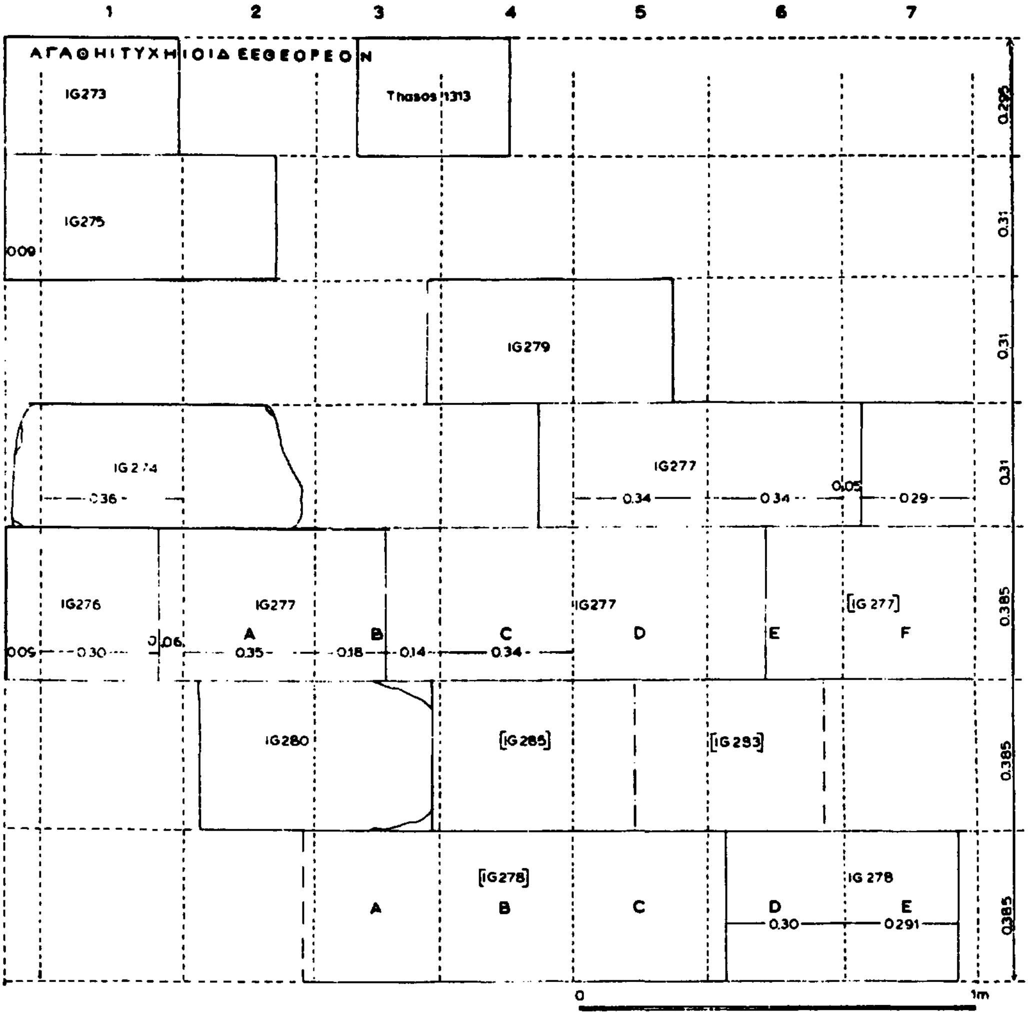

Figure 8. Salviat's fig. 6, reproduced by kind permission of the École Française d'Athènes

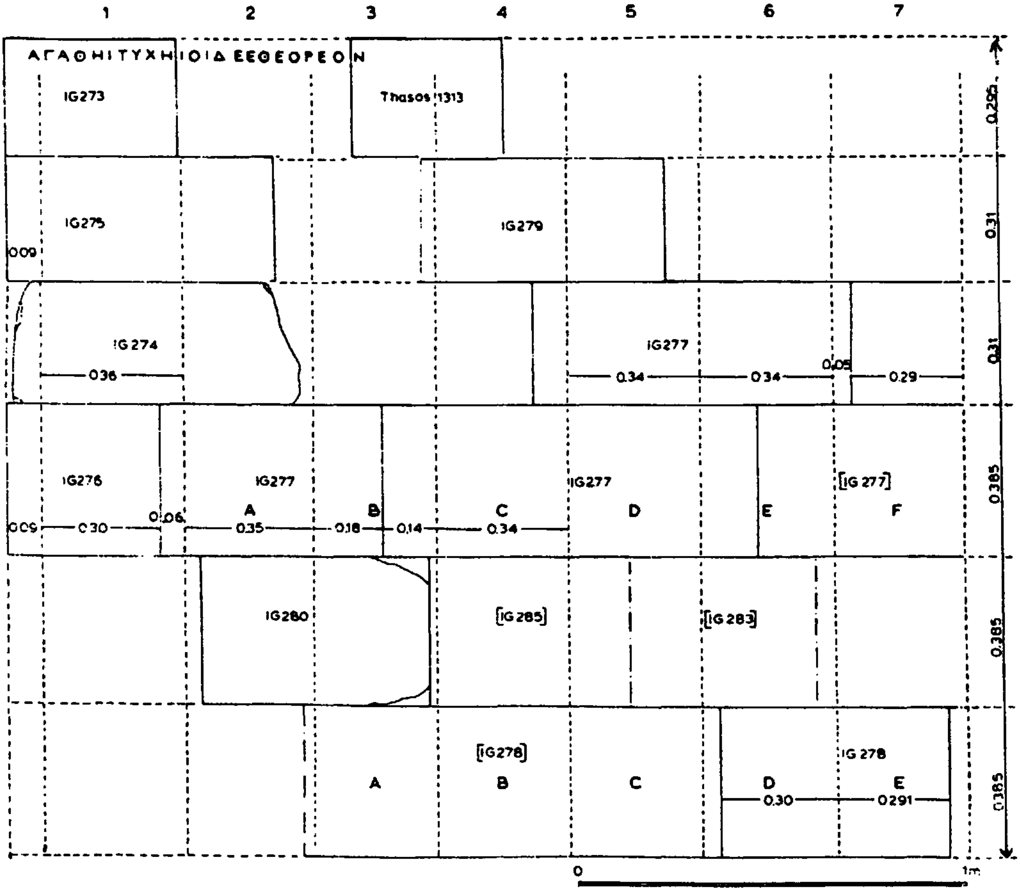

Figure 9. Salviat's fig.6, adapted to show the effects of removing his postulated third course of blocks 0.31 m. high

12. ΟΙΚΗΙΟΙ ΠΕΡΙΝΘΙΟΙ

The archaic inscription from Samos recording a dedication to Hera by two Perinthians (*SEG* xii 391) is a most interesting document from many points of view (Fig. 10). But in this paper I shall confine myself to a single topic, the crux of ll. 3-4.

Klaffenbach, the first editor,[1] frankly admitted that he could not master the problems presented by the letters on the stone (3f.) O.KHIHIOI. He was naturally reluctant to postulate dittography. The text is short and well inscribed, but that is not all. For when one has assumed the engraver's error one is still left with a phrase qualifying the names of the two dedicators, οἰκήιοι Περίνθιοι, for which Klaffenbach could see no satisfactory explanation. He therefore inclined to the possibility that there is a word for some form of craftsman concealed in these letters, and the dedication is comparable to the tithe dedications by potters from the Athenian acropolis.[2] The lack of any word for a craft at all similar to the letters on the stone gives this suggestion the appearance of a last resort.

Robert thought that dittography should be assumed and οἰκήιοι explained as referring to the relationship as kinsmen which existed between the two dedicators but which was not evident from their names and patronymics.[3] This explanation did not convince Klaffenbach,[4] nor is it supported by other examples. And, as Guarducci rightly objected,[5] clearer ways of expressing their relationship, if this is what the dedicators intended, could surely have been found.

A new approach to the problem was offered by Guarducci.[6] Undeterred by Klaffenbach's statement "es ist zwar verführerisch, wenn wir auf einer samischen Inschrift vor Perinthioi ein mit οἰκ. beginnendes Wort finden, darin ein Hinweis auf das Verhältnis der

[1] *Mitteilungen des deutschen archäologischen Instituts* vi (1953) 15-20. His text is reproduced as *SEG* xii 391.

[2] On all this see Klaffenbach, 17f.

[3] See Klaffenbach, *ibid.*, and *REG* lxxii (1959) 225 §320.

[4] *Loc. cit.*

[5] P. 25; see next note.

[6] Studi in onore di Aristide Calderini e Roberto Paribeni i (Milan, 1956) 23-7, "Un' antica offerta alla Era di Samo."

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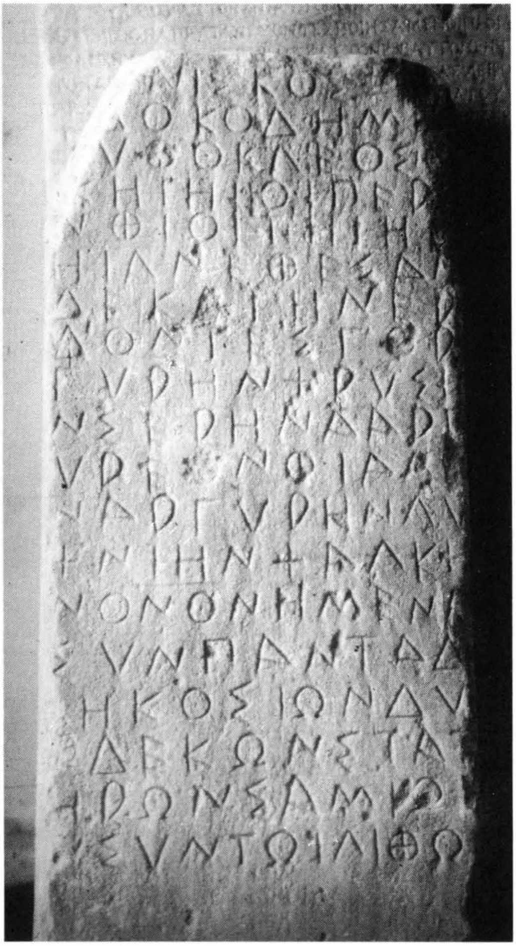

Fig. 10. ΟΙΚΗΙΟΙ ΠΕΡΙΝΘΙΟΙ.

beiden Städte zueinander zu vermuten, also eine Bezeichnung wie οἰκισταί, οἰκιστῆρες o.ä., aber die erhaltenen Buchstaben schilessen doch offenbar jede Möglichkeit der Bildung eines entsprechenden Wortes aus," she proposed to read a word, οἰκητήιοι, which she regarded as expressing the relationship of the Perinthians as colonists of Samos. She sought epigraphical justification for her reading in certain lines observable in the photograph, but even without these was prepared to argue that an engraver's error of ι for τ is a lesser assumption than dittography. No one would deny this if a satisfactory reading resulted. The proposed word οἰκητῆιος would be a ἅπαξ

λεγόμενον, which Guarducci forms from οἰκητής (translated colonist) on the analogy of the word οἰκετεία (οἰκετῆια), a body of slaves, formed from οἰκέτης. The two dedicators are, on this interpretation, described as colonising Perinthians, or, if we give the adjective the force of a noun, Perinthian colonists. Apart from the dubious method by which an engraver's error is assumed in order to produce a ἅπαξ λεγόμενον, the question remains: why should an unparalleled adjective be used when there were common nouns available meaning colonist and settler? It seems unanswerable, and Robert treated the suggestion with scorn.[7]

A brief mention of the problem of ll. 3-4 by L. H. Jeffery in *Local Scripts of Archaic Greece* is vitiated by the assumption that οἰκεῖος can mean colonist, and a misrepresentation of Klaffenbach's views by which he is made to agree with Guarducci.[8] ||

Of these treatments only Guarducci's attempts to correct Klaffenbach's actual reading. If one studies the stone itself[9] one is bound to conclude that Klaffenbach has rendered the letters on the stone entirely correctly. The apparently suggestive cuts round the second *omikron* in ο[ι]*κηιηιοι* are shallower than those of the letters and irregular, and though the gap of *c.* 0.05m. between the last two *iotas* looks wide it is in fact not significantly larger than that usually left for *omikron*. Apart from these cuts the letters on the stone are absolutely clear.[10]

Since there is no justification from the stone for any different reading, there is no escape from a choice of evils: either we assume dittography or an engraver's mistake. The great obstacle to the latter assumption is shown by Guarducci's attempt. No small or single change will produce a satisfactory reading. If we are to make either a word for settlers or founders or for Klaffenbach's suggested craft we have to postulate such considerable changes that we go far beyond any probable engraver's mistake. It is not surprising that Robert

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[7] *Loc. cit.*

[8] See 365 with n. 4. Cf. "This would mean that the phrase *οικηι{ηι}οι* means not 'the settlers' (i.e. original colonists) but simply 'the colonists'." Her own tentative suggestion that the two dedicators were "οικηιοι in the sense of Perinthian officials responsible for the gifts in a Perinthian οἶκος or Treasury" seems too far from the attested meanings of οἰκεῖος to be a serious possibility.

[9] To do this one needs now to visit the 'apotheke' by the main church in Tigani (officially Pythagoreion).

[10] See plate II.

reiterated his view that it is best to correct a 'simple dittographie'.[11] This is certainly the easiest assumption epigraphically.

But the failure till now to find a satisfactory interpretation shows that the problem is not to be settled merely by looking at the stone and a lexicon. The circumstances of the dedication should have weight in the choice of an explanation. Perinthos was a colony of Samos, and so Guarducci was surely right to look for a reference to this relationship in the word qualifying the two Perinthian dedicators, even if her actual solution is unacceptable. She very justly compared the dedication with other known instances of dedications by colonies to the deities of the metropolis.[12] It also seems an attractive explanation of the unusual[13] statement of the cost of the dedication that the two men were chosen to make the offering on the part of the state and the sum mentioned was the size of dedication required of Perinthos.[14] If this is right it meets Robert's objection that the analogy of other colonial dedications is inadmissible because this dedication is by individuals.[15] But even if it is wrong, dedications by individual colonists to the gods of the metropolis are known elsewhere.[16] It seems to me that unless one assumes that the Perinthian dedicators were consciously making an offering as colonists to the great deity of their mother city, one is setting one's face against both analogy and probability.[17]

A satisfactory solution will therefore combine the simplest epigraphical assumption with the most likely sense, viz. read o[ἰ]κῆι{ηι}οι and understand it as referring to the colonial relationship.

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[11] *REG* lxxii (1959) 225 §320. I take the liberty of correcting Robert's actual words 'simple *haplographie*'.

[12] See Guarducci, 26f., for examples.

[13] Diehl *Anth. Lyr. Graec.* ii (1925), Simonides no. 114, a dedicatory epigram probably not by Simonides (see M. Boas, *De Epigrammatis Simonideis* (Groningen, 1905) 119), is hardly an exact parallel, as the sum mentioned is presumably the sculptor's fee, not the cost of the dedication:

Ἀρτέμιδος τόδ' ἄγαλμα—διηκόσιαι γὰρ ὁ μισθὸς
δραχμαὶ ταὶ Παρίαι, τῶν ἐπίσημα τράγος—
ἀσκητὸς δ' ἐποίησεν Ἀθηναίης παλάμῃσιν
ἄξιος Ἀρκεσίλας, υἱὸς Ἀριστοδίκου.

[14] I also agree with her that this would explain why the two men dedicate together even though they were apparently not related.

[15] *Loc. cit.*

[16] E.g. that of Deinomenes to Athena Lindia; see *Lindian Chronicle* xxviii in Blinkenberg, *Lindos* ii.

[17] Whatever the date of the inscription, though the argument is stronger if it is dated to within a generation of the colony's foundation in *c.* 600, i.e. 580-570. This

The only meaning of οἰκεῖος likely to concern us here is 'kin, related' (section II.i of the article in *LSJ*). That the word could bear the meaning kin in a general sense can ‖ already be seen in Herodotus' order (viii 39.1) παῖδας . . . καὶ ἀδελφεοὺς καὶ οἰκηίους καὶ φίλους, and this character made it possible in Hellenistic times for οἰκεῖος to mean kin of a wide and indeterminate kind. The statement of the Epidamnians in their decree passed in reply to the representations of the people of Magnesia on the Meander, παρεκάλουν δὲ καὶ ἁμὲ ὄντας οἰκείους καὶ φίλους [ἀ]ποδέξασθαι τάν τε θ[υσί] αν κτλ.[18] (Ditt. *Syll.*^{3} 560.20f.), is a sufficient and by no means isolated example.[19] But such a wide use can also appear earlier; *cf.* Thuc. iv 64.3, where Hermocrates is made to say at the conference of Gela: οὐδὲν γὰρ αἰσχρὸν οἰκείους οἰκείων ἡσσᾶσθαι, ἢ Δωριᾶ τινὰ Δωριῶν ἢ Χαλκιδέα τῶν ξυγγενῶν, τὸ δὲ ξύμπαν γείτονας ὄντας καὶ ξυνοίκους μιᾶς χώρας κτλ. In general we may conclude that οἰκεῖος would be a satisfactory word for Perinthians to use to describe their relationship to Samos.

However, the two Perinthians, if they wanted to express this, chose to do so elliptically. Where the reference of οἰκεῖος is obvious it is frequently not expressed, and in archaic inscriptions it was not thought necessary to state the self-evident, but these general truths are hardly enough in themselves. An analogy may help. The ὅρκιον τῶν οἰκιστήρων of Cyrene[20] contains the phrase Αἰ μὲν δέ κα κατέχ[ων]τι τὰν οἰκισίαν οἱ ἄποικοι, τῶν οἰκείων τὸγ καταπλέον[τα] ὕστερον ἐς Λιβύαν καὶ πολιτήιας καὶ τιμᾶμ πεδέχ[εν] καὶ γᾶς τᾶς ἀδεσπότω ἀπολαγχάνεν (30-33).[21] Here too the reference of οἰκεῖος is not stated and different interpretations

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is where Klaffenbach placed it (16f.), but Jeffery offers a much later date, *c.* 525 (371, no. 35), on arguments which do not seem to me compelling; see 365.

[18] Cf. 3f.: [ἐπειδὴ Μ] άγ[νητες] οἱ ἐπὶ Μαι[ά]νδρ[ου] συγγ[ενεῖς] ὄντες καὶ φίλοι τῶν Ἐπιδαμνίων.

[19] Robert makes the general comment that in Hellenistic decrees οἰκεῖος is sometimes equivalent to συγγενής but not always (*BCH* lii (1928) 171 n. 5).

[20] *SEG* ix 3. I discuss this document in *JHS* lxxx (1960) 94ff.

[21] The text is that of Oliverio, *Riv. di Fil.* vi (1928) 225. The words τῶν οἰκείων were printed by him in large type to indicate that he read them with difficulty (cf. 186). Mr P. M. Fraser has kindly informed me (by letter) that he could not see the letters ι ἄποικοι, τῶν οἰκείων on the stone, which is now fixed in a dark part of the museum. However, Oliverio (223) describes how he waited for the sun to strike the stone at favourable angles to reveal each letter in turn, and it is noteworthy that even his photograph (pl. 12) reveals two letters quite clearly (ων of οἰκείων) which were not visible to Mr Fraser, and possibly others (e.g. π of ἄποικοι). There is, therefore, sufficient reason for confidence in Oliverio's reading of this passage. It is not among those attacked by Ferri, *Historia* iii (1929) 389-96.

have been put on the word by modern scholars.[22] But the ancients at least had no doubts. The ancient foundation decree is given as justification for the Cyrenean action in conferring citizenship on all the Theraeans; the only part of it which could justify such an action is this phrase; so it was clearly understood to mean all the Theraeans. In my opinion we cannot say for certain whether a given part of the wording of the ὅρκιον is authentic; so it is not possible to decide whether οἰκείων is part of the original wording of the seventh-century decree, though this does not seem impossible.[23] In the fourth century at least the word was regarded as sufficient in itself to mean kinsmen of the colonists, i.e. all citizens of the metropolis. This suggests that οἰκεῖος had a clearly recognized meaning in the context of the relations between a colony and its mother city: the citizens of each were οἰκεῖοι of the other. Hence two Perinthian dedicators who set up a stele in the precinct of the great goddess of their metropolis could call themselves 'Perinthian kinsmen' without further elaboration and so express their relation as colonists to Samos. ||

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[22] E.g. Oliverio and Chamoux took it as equivalent to all the Theraeans (*Riv. di Fil.* vi (1928) 227, and *Cyrène sous la monarchie des Battiades* (Paris, 1952) 107), while Meiggs' translation suggests to an English reader a more narrow sense of relationship ('any kinsman'; see Bury, *History of Greece*³ 862).

[23] I discuss the question of the decree's authenticity at length in the article cited above, n. 20. Cf. also Jeffery, *Historia* x (1961) 139ff., whose conclusions, reached independently, are on the whole similar. I take this opportunity to acknowledge the justness of Parke's criticism of my treatment of the word αὐτοματίζω (*JHS* lxxxii (1962) 145).

13. ADOPTED TEIANS: A PASSAGE IN THE NEW INSCRIPTION OF PUBLIC IMPRECATIONS FROM TEOS

The new inscription of Public Imprecations from Teos, apart from many other interesting features, represents what is surely the most important new evidence to accrue for a generation on the relations between Greek colonies and their mother cities.[1] The inscription was admirably published by P. Herrmann in the editio princeps,[2] and helpful contributions followed from Merkelbach[3] and Lewis[4] before its republication in *SEG* xxxi (1981; appeared 1984) 965, and, most recently, by McCabe and Plunkett.[5]

In his edition Herrmann did not venture a reading for the passage A. 6-7, even though the gaps are small and there is no doubt about the letters preserved on the stone. Although he saw that it was possible to read [σ]ὺν θετοῖσιν [T]ηΐ[ο]ι[σ]ιν, his inability to see a good sense led him to print: [.]ΥΝΘΕΤΟΙΣΙΝ.ΗΙ.Ι.ΙΝ:.[6] Merkelbach suggested that the reading should be [σ]ὺν θετοῖσι (sic) [T]ηΐ[ο]ι[σ]ιν, yielding the complete phrase ὃς ἂν τιμὴν ἔχων [σ]ὺν θετοῖσι [T]ηΐ[ο]ι[σ]ιν τὸμ πλησίον δολῶται, τοῦτον ἀπόλλυσθαι καὶ αὐτὸν καὶ γένος τὸ κένο, which he translated "Wer als Magistrat mit adoptierten (= neueingebürgerten) Teiern den Nachbarn unterjocht, der soll zugrunde gehen, er selbst and sein Geschlecht."[7] Although this reading is attractive epigraphically, since it involves no change in the letters preserved

[1] I suppose the decrees encouraging delation from Thasos (ML 83), first published by J. Pouilloux in 1954 (*Recherches sur l'histoire et les cultes de Thasos*. I, Paris), was the last new evidence of comparable importance to appear. The scholarly world is still awaiting the publication of an inscription from Naupactus, which is apparently of at least equal significance, to judge by the tantalizing description by Mastrokostas in *Arch. Delt.* xix.2 (1964) 295.

[2] "Teos und Abdera im 5. Jahrhundert v. Chr.," *Chiron* xi (1981) 1-30 (hereinafter Herrmann).

[3] 'Zu dem neuen Text aus Teos', *ZPE* xlvi (1982) 212-13.

[4] 'On the new text of Teos', *ZPE* xlvii (1982) 71-2.

[5] Donald F. McCabe and Mark A. Plunkett, *Teos inscriptions* (Princeton 1985) 262. This is the most recent published corpus of the inscriptions of Teos.

[6] See Herrmann, 6, 14f. and plate 1. Herrmann's success in deciphering large parts of this inscription with virtually complete certainty was deservedly praised by Merkelbach, 212.

[7] *Ibid.* Although Merkelbach omits the final nu of θετοῖσιν, it was clearly read by Herrmann, and we may henceforward silently correct this obvious slip.

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and offers easy, acceptable supplements for those missing, it was not adopted by the editors of *SEG* nor by McCabe and Plunkett, who reprinted Hermann's text.

The adjective θετός is a standard word for 'adopted' (adopted child, son, daughter, father etc.) attested from Pindar[8] and Aeschylus[9] down to Byzantine times,[10] and is glossed εἰσποιητός.[11] The basic meaning is 'made' as opposed to 'born', 'natural'.[12] The only parallel for 'adopted' citizens that I have found is *Anth. Graec.* vii. 418.4, where the adjective is used with ἀστός:

Πρώτα μοι Γαδάρων κλεινὰ πόλις ἔπλετο πάτρα,
ἠνδρωσεν δ' ἱερὰ δεξαμένα με Τύρος·
εἰς γῆρας δ' ὅτ' ἔβην, ἁ καὶ Δία θρεψαμένα Κῶς
κἀμὲ θετὸν Μερόπων ἀστὸν ἐγηροτρόφει.

However, even without contemporary parallel, the word's significance and usage make Merkelbach's understanding of it here entirely acceptable. The question remains what historical circumstances lay behind this description.

In none of the publications given above was there any mention of Pindar's Second Paean. This works make allusions to several events in the early history of Abdera, the noted colony of Teos, whose appearances in the new inscription constituted its greatest surprise. One of these allusions runs as follows:

νεόπολίς εἰμι· ματρὸς
δέ μάτερ' ἐμᾶς ἔτεκον ἔμπαν
πολεμίωι πυρὶ πλαγεῖ-
σαν·

This is easily translated,[13] but the historical interpretation caused difficulty to early commentators, and unsatisfactory solutions long held the field. However, these were all swept away by the excellent and entirely convincing interpretation of Radt in his edition of the

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[8] *Ol.* 9.62.

[9] *Fr.* 320.

[10] I rely on an 'all Greek' search of the *TLG* material by Ibycus computer, which was kindly performed for me by my friend and colleague, Professor Wesley Smith.

[11] Harpocration, s.v. θέτης.

[12] For the verbal use from which the adjective comes, see LSJ s.v. τίθημι 3.b.

[13] "I am a young city; yet I gave birth to the mother of my mother, when she was smitten by the foeman's fire" (With acknowledgements to the translations of Sandys in the Loeb edition and of S. L. Radt, *Pindars zweiter und sechster Patian* [Amsterdam 1958] 22f.).

poem,[14] and that interpretation was rightly followed by G. Huxley.[15] Radt demonstrated that 'my mother' is Abdera, 'the mother of my mother' is Teos, and the passage thus provides evidence of refounding of Teos by Abdera, which is not explicitly attested in any other extant literature.

Both Radt and Huxley discussed when this refounding took place. The two most obvious || possibilities are soon after Harpagus' conquest in *c.* 545 and after the end of the Ionian revolt in *c.* 493. In favour of the former are Strabo's statement that some of the colonists of Abdera later returned to Teos,[16] the fact that Teos was a substantial enough city to provide 17 ships at the battle of Lade,[17] and its early coinage. The first silver coins of Teos are contemporary with those of Abdera, and Abdera's were struck soon after its colonization by the refugees from Teos.[18] Furthermore, the precise similarity of the observe type, except that Teos' griffin is turned to R. Abdera's to L, "suggests that the coinages were started in planned conjuction."[19] So Teos existed again soon after the total evacuation of the city at the time of Harpagus' conquest,[20] and close relations with Abdera can be assumed.

However, the second possibility, after the Ionian revolt, is also attractive. Herodotus' description of the burning of the rebel Ionian

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[14] *Op. cit.* 33-9 (including information about earlier scholarship). It is regrettable that B. Isaac reverts to earlier interpretations in his recent book. *The Greek settlements in Thrace until the Macedonian conquest* (Leiden 1986) 90-2. Radt's refutation of these ideas was decisive.

[15] 'Teos in Pindar,' *Studies presented to Sterling Dow on his eightieth birthday* (Durham, N.C. 1984) 149-52.

[16] xiv 1.30 (C644).

[17] Hdt. vi 8.1.

[18] C. M. Kraay, *Archaic and classical Greek coins* (London 1976) 35, 152; plate 53, 893-5; plate 30. The downdating of the first coins of Abdera to *c.* 530-500, described by M. J. Price and N. Waggoner as 'inescapable' (*Archaic Greek coinage: The Asyut hoard* [London 1975] 37), is based, as often in that book, on a misconception of the chronological significance of Egyptian bullion hoards; *cf.* what the authors themselves say, 117. And L. H. Jeffery's argument that the letters on the coins are surprisingly advanced for *c.* 540 is clearly a priori (*Local scripts of Archaic Greece* [Oxford 1961] 364). So it is unfortunate that B. Isaac has accepted the downdating; *op. cit.* 87-9. Kraay was right to follow J. M. F. May, *The coinage of Abdera* (London 1966), who took account of some good hoard evidence (51-3), which is perfectly consistent with the earlier dating. May also understood (49) that, just as at Elea (Kraay, 170), colonists familiar with coinage would not delay long in introducing it in their new home.

[19] Kraay, 35.

[20] Note the πάντες of Hdt. i 168.

cities by the Persians[21] makes a very good fit with Pindar's πολεμίωι πυρὶ πλαγεῖσαν, and the circumstances of the Ionian revolt seem also to suit better the gnome with which Pindar follows the allusion to the refoundation:

> εἰ δέ τις ἀρκέων φίλοις
> ἐχθροῖσι τραχὺς ὑπαντιάζει,
> μόχθος ἡσυχίαν φέρει
> καιρῶι καταβαίνων.[22]

While nothing can be pressed in such a vague generalization, if the choice is between the period following Harpagus' conquest and the end of the Ionian Revolt, the latter seems more apposite.[23] In addition to these arguments, the new inscription from Teos offers a further line of approach.

The ban against setting up an *aisymnetes* (A. 22-4) has shown the correct interpretation of a parallel provision in the long-known similar inscription from Teos.[24] Both may be seen as forbidding the establishment of a tyrant, whose (possibly euphemistic) title at Teos was *aisymnetes*. Like the other Ionian cities, Teos may be assumed to have been ruled by pro-Persian tyrants before and, briefly, after the Ionian revolt.[25] As Herrmann pointed out,[26] the new inscription may well imply recent, severe political upheaval. The fall of the Ionian tyrants at the beginning of the Ionian revolt, their replacement by democracies, the re-establishment of tyranny at the end of the revolt, and its final demise in favour of democracy at the instigation of Mardonius,[27] obviously represent such severe political upheaval. Such circumstances offer a likely occasion for bringing in new citizens.[28]

It is difficult to choose between these two possibilities. Radt thought Abdera could have helped the mother city on both occasions, and here too he is followed by Huxley. However, Pindar's words seem to exclude the possibility of two refoundations, since they clearly imply

[21] vi 32.

[22] ll. 31-4: "And if in helping one's friends a man ruggedly faces the enemy, peace is brought by toil which comes at the right time."

[23] As Radt, 38. For the gnome, see his commentary, 39-42.

[24] *ML* 30; See SEG xxxi (1981) 984.

[25] See Hdt. v. 7-8; vi 43.4.

[26] Pp. 24, 29f.

[27] Hdt. *loc. cit.*; cf. also iv 137.2-138.

[28] It is relevant here that Herrmann would date the new inscription, on the basis of letter forms, to *c.* 480-450, preferably closer to the lower terminus (p. 6).

a single act. Although my initial preference[29] was for the time after the Ionian Revolt, on the grounds that there would be less reason for pride if substantial numbers of the original settlers had (like the Phocaeans)[30] returned to Teos. I now incline to the view[31] that we have insufficient evidence to choose between the two possibilities, or even, strictly, to exclude other occasions unknown to us. The one certainty is that the refoundation occurred at some time within the early history of Abdera.

If Abdera sent to Teos a sufficient number of its citizens to refound the mother city, we seem to have here possible candidates for the 'adopted Teians' of the inscription. And if they were sufficiently numerous to represent a refoundation, it may not be surprising that the previous inhabitants of Teos entertained fears that they might suffer disadvantages at the hands of the newcomers. One remembers the fate of the old Sybarites at Thurii.[32] Presumably it was the fear of such a specific contingency that led to the inclusion of a provision apparently somewhat illogical: we can hardly believe that a magistrate who enslaved his fellow citizens *without* the help of the new citizens would be exempt from the curse. It may be, however, that this possibility was covered by the general undertakings that immediately follow (A. 10ff.).

Naturally we cannot prove that the 'adopted Teians' of the inscription are the newcomers from Abdera who refounded their mother city, and there is one possible objection to such an identification. The presence of Abdera in the new inscription might be held to show that some form of *sympoliteia* existed between the colony and the || mother city.[33] If relations were so close, would Abderites who refounded Teos properly be called 'adopted Teians'? We lack the necessary parallels to provide a sure answer to such a question. So here too we must acknowledge uncertainty.

Even so, this discussion has shown possible historical circumstances which would justify Merkelbach's reading of A. 6-7, and even if they

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[29] *Colony and mother city in Ancient Greece*, 2nd ed. (Chicago 1983), 'Addenda and Corrigenda', p. xxxi, no. 34.

[30] Who broke their oath: Hdt. 1.165.3.

[31] Cf. Radt, 38.

[32] Diod. xii 11.1-2.

[33] *See* Herrmann's discussion, 26-30, though he does not hazard a political definition. N. Ehrhardt tentatively allows the possibility of *sympoliteia*; see *Milet und seine Kolonien* (Frankfurt, Bern, New York, 1983) 234.

are not its specific justification, at least we know that Teos in this period had need of new citizens. Thus the historical background, either specifically or generally, supports a reading which was already very satisfying epigraphically. It should be promoted to the text.[34] ||

[34] I am very grateful to my friend and colleague Professor Martin Ostwald for kindly reading this note in draft and making several helpful suggestions for its improvement.

14. ABDERA AND TEOS

Introduction

Abdera and Teos have been brought into prominence recently by the discovery of the new inscription of Public Imprecations from Teos, the provisions of which refer not only to Teos but also to its great colony.[1] But there are additional reasons why these two cities deserve fresh consideration. The excavations at Abdera, which have now been conducted over many years, may in some respects seem disappointing;[2] no temples and, apart from the theatre, no public buildings have come to light; but they nevertheless require important changes in our interpretation of the history of the city. Secondly, there are many allusions to the early history of Abdera in Pindar's Second Paean, composed for that city. Their use by historians was long hindered, not only by their inherent obscurity, but also by unsatisfactory reading and interpretation of the Greek. The second of these hindrances was removed by the fine edition of the poem published by S. L. Radt in 1958.[3] Even so, this important evidence seems in general not to have entered the consciousness of ancient historians.[4]

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[1] *SEG* xxxi (1981) 985; Donald F. McCabe and Mark A. Plunkett. *Teos inscriptions* (Princeton 1985) 262: admirable *editio princeps* with rich commentary by P. Herrmann, "Teos und Abdera im 5 Jahrhundert v. Chr." *Chiron* xi (1981) 1-30.

[2] *Cf.* the perhaps rather intemperate criticisms of the strategy of excavation by K. A. Dimadis, "Le théâtre d'Abdera," *Balkan Studies* xv (1974) 308-21.

[3] *Pindars zweiter und sechster Paian* (Amsterdam). I refer to this work henceforth by the author's name alone.

[4] With the honourable exception of G. Huxley ('Teos in Pindar', *Studies presented to Sterling Dow on his eightieth birthday* [Durham NC 1984] 149-52), commentators on the new inscription of Public Imprecations from Teos (n. 1) did not mention Pindar's Second Paean, though it is obviously relevant: see my "Adopted Teians: a passage in the new inscription of Public Imprecations from Teos," *JHS* cxi (1991) 176-8. It is notable too that B. Isaac, although he cites Radt's work, reverts to earlier interpretations which Radt decisively refuted: see *The Greek settlements in Thrace until the Macedonian conquest* (Leiden 1986) 86, 90-3, for example. Earlier J. and L. Robert showed by the interpretation they accepted that they were unaware of Radt's work: see "Une inscription grecque de Téos en Ionie. L'union de Téos et de Kyrbissos," *Journal des Savants* (1976) 153-235, at 213 n. 238.

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The Name Abdera

Any consideration of Abdera's history must begin with its Phoenician name. There are two other cities from Antiquity called Abdera. One was in North Africa, near Carthage,[5] and the other was on the South coast of Spain.[6] This latter is expressly attested as Phoenician, and issued coins with its name in Phoenician letters, or, sometimes, written twice in both the Phoenician and the Roman alphabet.[7] So there is no doubt that the name Abdera is Phoenician, and this has long been recognized by good judges.[8]

We should not follow those, therefore, who see the name (in whole or in part) as local Thracian or as Greek. Thus Wilamowitz argued very wilfully that the local epithet of Apollo, Δηρηνός/Δηραινός, could not be a place-name, as the scholiast to Lycophron. *Alex.* 440, stated: the actual place-name must be Δηρος or Δηρα, the second part of Abdera.[9] Apart from denying || the plain statement of the scholiast, this explanation ignores the fact that we are dealing with a toponym also found in two Phoenician areas of the Western Mediterranean. As for the view of D. K. Samsaris that Abdera was derived from Abderos, the Greek eponymous hero, rather than vice versa,[10] that is simply an offence against elementary rules.

Even though Abdera is definitely a Phoenician name, it has resisted successful etymological analysis.[11] The regular form of the Phoenician name on the coins of Spanish Abdera consists of the five letters (from right to left) ayin, beth, daleth, resh, taw, ABD-R-T. Coins bearing only the four letters, ayin, beth, daleth, resh,[12] were probably the

[5] Ptol. *Geog.* iv 3.9; *cf. RE* s.v. Abdira.

[6] Strabo iii 4.3, C157, among many other mentions: *cf. RE* s.v. Abdera, 2.

[7] See A. Vives y Escudero, *La moneda hispanica* (Madrid 1924) 3, 16-19, plate LXXXI and, for a more recent treatment, G. K. Jenkins, *Sylloge Nummorum Graecorum* 43, *Copenhagen* (Copenhagen 1979) Spain, 67, 68.

[8] E.g. E. Meyer, *Geschichte des Altertums* ii.2[2] (Stuttgart and Berlin 1931) 93; V. Bérard, *Les Phéniciens et l'Odyssée* (Paris 1903) ii 20f.; A. Dietrich, "Phönikische Ortsnamen in Spanien," *Abhand. für die Kunde des Morgenlandes* xxi.2 (Leipzig 1936) 10f., and, recently, Isaac, *op. cit.* 76f.

[9] *Sappho und Simonides* (Berlin 1919) 255f. For a similar idea, though not developed, see also C. van Holzinger in his edition of Lycophron (Leipzig 1895) 237. On the epithet and evidence about it, see Radt, 29f.

[10] Ὁ ἐξελληνισμὸς τῆς Θρᾴκης κατὰ Ἑλληνικὴ καὶ Ῥωμαικὴ ἀρχαιότητα (Thessalonike 1980) 240.

[11] A. Dietrich's statement to this effect (n. 8) is unfortunately still true.

[12] E.g. George Macdonald, *Catalogue of Greek coins in the Hunterian Collection* iii

results of faulty minting.[13] The single six-lettered example (ayin, beth, daleth, resh, ayin, taw, ABD-RAT), reported as being in Paris by nineteenth-century scholars, can no longer be found in the Cabinet des Médailles.[14] Although not now strictly attested in the Phoenician alphabet, the vowels between D and R and R and T seem assured by the Greek and Latin spelling, Ἄβδηρα, Abdera. And the ending in AT may be the feminine ending, of which the T was lost in the transliterations.[15]

The first element in the name, ABD, would be recognized by Phoenician speakers as meaning slave/servant, a very common initial element in personal names, which is normally followed by the name of a deity.[16] But no satisfactory explanation of the second element, ERA(T), has been found, even though it is worth noting that similar endings are found in other Phoenician place-names, such as Gadeira, Cythera.[17] However, it would be inappropriate for a non-expert to pursue these questions here, especially as even the first element of Abdera presents the difficulty that it is not obviously suitable in a place-name.[18]

We must content ourselves, therefore, with the basic fact that Abdera was named by Phoenicians, and that occurred before *c.* 654 BC, the date of the first Greek attempt to colonize the place. The presence of Phoenicians in the area at that time is attested by

(Glasgow 1905) 658. I am very grateful to Dr J. D. Bateson for kindly confirming the reading of the two coins in question and for sending me photographs.

[13] This is the opinion of G. K. Jenkins (n. 7).

[14] E.g. T. E. Mionnet, *Description des médailles antiques, grecques et romaines* (Paris 1806-37) Suppl. i, plate 5 no. 1; W. Gesenius, *Scripturae linguaeque Phoeniciae monumenta quot supersunt* (Leipzig 1837) 311 and plate 41, XVII, H. M. Jean-Baptiste Giard kindly informed me that this coin cannot be found in the Cabinet des Médailles.

[15] See S. Segert, *A grammar of Phoenician and Punic* (Munich 1976) 87; Zellig Harris, *A grammar of the Phoenician language* (New Haven, CT 1936) 58f. I am grateful to the Reverend Professor J. A. Emerton for guidance on this and other matters of language.

[16] *Cf.* e.g. Frank L. Benz, *Personal names in the Phoenician and Punic inscriptions* (Rome 1972) 148-64.

[17] Gadeira, Hdt. iv 8.2. e.g.; Cythera, Hdt. i 105.3; *cf.* J. P. Brown, "Kothar, Kinyras and Kytheria," *Journ. Semitic Studies* x (1965) 197-219, at 209.

[18] Thanks are expressed here to all those not mentioned in previous notes, who generously helped me in a field in which I am not expert: on the coins, Dr C. Arnold-Biucchi (American Numismatic Society), Dr M. J. Price (British Museum) and Mr T. Volk (Fitzwilliam Museum); on matters of language, Dr Y. L. Arbeitman, Professor J. Pritchard, Dr J. D. Ray.

Herodotus for Thasos,[19] and their interest in Abdera is easily explained by the existence in the region of deposits of precious metals.

The Colony of Clazomenae

Until recently almost the only valuable information about the colony of Clazomenae at Abdera was contained in Herodotus' brief sentence (i 168):

> παραπλήσια δὲ τούτοισι καὶ Τήιοι ἐποίησαν· ἐπείτε γάρ σφεων εἷλε χώματι τὸ τεῖχος Ἅρπαγος, ἐσβάντες πάντες ἐς τὰ πλοῖα οἴχοντο πλέοντες ἐπὶ τῆς Θρηίκης καὶ ἐνθαῦτα ἔκτισαν πόλιν Ἄβδηρα, τὴν πρότερος τούτων Κλαζομένιος Τιμήσιος κτίσας οὐκ ἀπόνητο, ἀλλ' ὑπὸ Θρηίκων ἐξελασθεὶς τιμὰς νῦν ὑπὸ Τηίων τῶν ἐν Ἀβδήροισι ὡς ἥρως ἔχει.[20]

It seems likely that Herodotus is giving a brief summary of a more detailed story that he knew; it was not only Thucydides who could be rigorously selective in what he tells us.[21] In any case, on the basis of Herodotus' statement the Clazomenian venture has been looked on as a colonizing failure, which did not establish a settlement.[22]

Of the remaining literary evidence relating to the colonization by Clazomenae the most valuable historically is Eusebius' foundation date, *c.* 654.[23] Late authors offer further information about Timesios. Plutarch has a foundation oracle: σμῆνα μελισσάων τάχα τοι καὶ σφῆκες ἔσονται.[24] And both he and, more briefly, Aelian relate that Timesios

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[19] Hdt. ii 44.3-4, vi 47.1-2; *cf.* my remarks in "The foundation of Thasos," *BSA* lxxiii (1978) 61-98, at 88-92. For the evidence on the ground of the mining mentioned by Herodotus in the second passage, see J. des Courtils, T. Kozelj, A. Muller "Des mines d'or à Thasos," *BCH* cvi (1982) 409-17.

[20] "The Teians also acted similarly to these men (the Phocaeans). For when Harpagus had captured their wall with the siege-mound, they all embarked in their ships and sailed away to Thrace, and colonized there the city of Abdera, which the Clazomenian Timesios had founded before them, but had had no joy of it. He was expelled by the Thracians and now receives honours as a hero from the Teians in Abdera." Although the Herodotean tradition spells the name of the *oikistes* Timesios, it appears as Timesias in later authors, such as those mentioned below. It is hardly possible for us to determine the correct form, but I shall follow the Herodotean tradition in this paper.

[21] D. Lateiner, *The historical method of Herodotus* (Toronto 1989) 59-75, discusses the explicit evidence for Herodotus' selectivity.

[22] My own earlier opinion: see *CAH* iii^2.3, 117.

[23] A useful table of Eusebian foundation dates was given by R. M. Cook in his "Ionia and Greece, 800-600 BC," *JHS* lxvi (1946) 67-98, at 77.

[24] *De amicorum multitudine* 96 B: "Swarms (LSJ s.v. σμῆνος (note the heteroclite plural)) of bees will soon be wasps."

became unpopular through doing everything himself, until, hearing a child express the hatred, he decided to leave the city.[25] The oracle was adjudged by Parke and Wormell to have been concocted in connection with the Teian foundation,[26] while the city that he left because of hatred has been thought to have been Clazomenae.[27] It can hardly be Abdera in view of Herodotus' expulsion by Thracians. Clearly there is nothing very solid here for the historian. Nor can any new information of value be extracted from Solinus' mythical Abdera founded by Diomedes' sister, which had fallen into ruins when it was refounded by Clazomenians in the 31st Olympiad.[28] So far as literary evidence goes, therefore, only Herodotus and the chronographers' foundation date seem of undoubted historical value.

The reason there is today more to say about Clazomenian Abdera is that archaeology has here made an important contribution. Of pre-Teian Abdera the spade has so far unearthed no evidence of native Thracian predecessors, nor of Phoenicians[29] (but the site is immense and only a fraction has so far been explored; see Fig. 12). However, a substantial quantity of Greek || graves, which date from before the Teian foundation, have been discovered.

These graves belong to the extensive North West cemetery. They are well dated by the pottery some of them contain, and those so far examined range in date over the whole of the second half of the seventh century and reach into the beginning of the sixth.[30] Even though there is as yet no certain evidence for the settlement inhabited by the occupants of these graves,[31] the cemetery by itself provides archaeological confirmation for the Eusebian foundation date, and, what is more surprising, shows that there was a sizable Greek settlement on the site for at least two generations after Timesios' colonization. ||

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[25] Plut. *Praecepta reipublicae gerendae* 812a-b; Aelian, *V.H.* xii 9.

[26] *A history of the Delphic oracle* (Oxford 1956) i 61.

[27] A. Demandt, *RE* Suppl. xiv s.v. Timesios; I. Malkin, *Religion and colonization in ancient Greece* (Leiden 1987) 54-6 (a full and perceptive discussion).

[28] C. Iulii Solini *Collectanea rerum memorabilium* (ed. Mommsen, Berlin 1864) 10.10.

[29] Ch. Koukouli-Chrysanthaki, "Abdera and the Thracians," *Thracia Pontica* iii (1986) 82-98, at 84.

[30] *Op. cit.* 82f.; E. Skarlatidou, "The Archaic Cemetery of Abdera," *Thracia Pontica* iii (1986) 99-108; Koukouli-Chrysanthaki, *Ergon* 1987, 17f.; *cf. Arch. Rep. for 1988-9*, 85.

[31] See Skarlatidou, *op. cit.* Although Koukouli-Chrysanthaki has suggested that the early wall circuit she has investigated may be Clazomenian, *Thracia Pontica* iii, 83f., she kindly told me in person that the wall is not certainly dated. No buildings dating to the period of the early cemetery have so far been discovered. I am also grateful to E. Skarlatidou for verbal information on these questions.

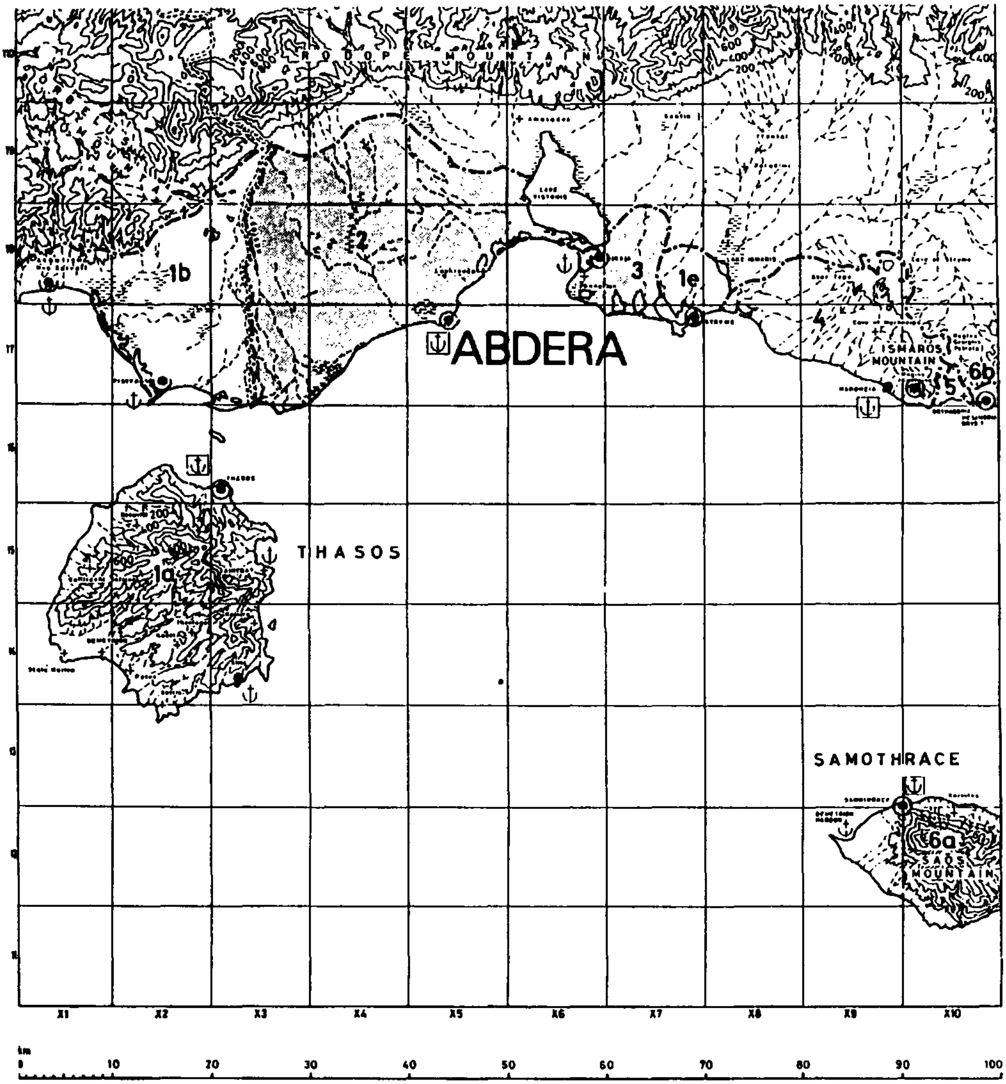

Figure 11. Abdera: position and presumed *chora* (after Lazarides, *Abdera and Dikaia*, fig. 21)

Grave evidence alone does not strictly allow us to say what Greeks these were, much less define the organization of their community. B. Isaac has suggested that Thasians settled the site between the Clazomenian and Teian colonizations.[32] Such a hypothesis cannot at present be disproved, but the arguments in its favour are not strong. They are: (1) the proximity of Thasos; (2) Archilochus' evidence for fighting against the Saioi;[33] (3) the Greek graves of the later part of the seventh century. Of these neither (1) nor (3) is sufficiently specific. As to (2), this Thracian tribe cannot be placed so narrowly as Isaac wishes ("between the Nestos and Lake Bistonis"). They held the mainland adjacent to Samothrace.[34] Archilochus' fighting goes more easily with the known Thasian attempt to win Stryme.[35] Only that attempt could be held to give some geographical support to Isaac's thesis, for otherwise the Thasian *peraea* is all to the north and west of the island. Finally, his hypothesis requires another hypothesis, that the presumed Thasian settlers of Abdera were driven out by the Thracians. So Isaac's suggestion is not attractive.

Occam's Razor would encourage us to assume that the Greek settlement at Abdera of the second half of the seventh century was the Clazomenian colony, since these are the only Greeks, other than the Teians, recorded as having colonized the site. Furthermore, the finds in the graves have been judged to point unmistakably to the Ionian origin and character of their occupants.[36] In that case, what about the expulsion of the *oikistes* by the Thracians? We should have to imagine that, while Timesios himself was expelled by the Thracians, at least some of his settlers were able to remain. It is to be noted here that Herodotus does speak only of Timesios himself. Perhaps we were wrong in the past to assume that Timesios' fate was shared by the whole colony.[37] There is no need to underline the uncertainty of such suggestions, but, until we obtain better evidence. I prefer to

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[32] *Op. cit.* (n. 4) 79f.

[33] Archilochus 5 West, the famous shield poem: ἀσπίδι μὲν Σαΐων τις ἀγάλλεται κτλ.

[34] Strabo x 2.17, C 457.

[35] The clearest evidence is Philochorus, *FGH* 328 F 43. For the correct geographical position of Stryme, i.e. West of Maronea, see G. Bakalakis, *Proanaskaphikes erevnes sti Thraki* (Thessaloniki 1958) 91-7.

[36] See Skarlatidou (n. 30) 105.

[37] Skarlatidou, *ibid.*, assumes that Herodotus was in error.

assume that the Greeks who lived at Abdera for at least the second half of the seventh century were Clazomenian colonists and their descendants. Others may incline more cautiously to await some better evidence, but even they could not deny that these early graves have substantially altered the history of settlement at Abdera.

The Teian Foundation

The really successful colonization of Abdera was that by the Teians, who fled their city *en masse* rather than be captured by the Persians in *c.* 545. Herodotus tells us this (i 168, the passage cited at the beginning of the previous section) as a brief appendage to his long account of the similar Phocaean action. The fact is well known and stated by other authors, such as Strabo (xiv 1.30, C644). Although not mentioned by Herodotus, we also know of the contemporary colonization by other refugees from Teos of Phanagoria on the Cimmerian Bosporus (Ps.-Scymnus 885f. Diller; Eustathius 549, *GGM* ii 324f.; *cf.* Hecataeus, *FGH* 1 F 212). While there is strictly no need for archaeological confirmation of such a well-established historical event as the Teian colonization of Abdera, the modern excavations have discovered a shrine at the north-west corner of the wall circuit, at which the earliest votives date from the third quarter of the sixth century.[38] So it seems likely that the life of the sanctuary began with || the arrival of the Teians. And nearby investigations of the city wall showed part to have been constructed in the second half of the sixth century.[39]

More detail about the events of the Teian colonization can be obtained, if with some difficulty, from the allusions in Pindar's Second Paean. It is clear from lines 59-70 that hard fighting was necessary to establish the colony:

τοὶ σὺν πολέμωι κτησάμ[ενοι
χθόνα πολύδωρον ὄλ[βον
ἐγκατέθηκαν πέραν Ἀ[θόω] Παιόνων
αἰχματᾶν [λαοὺς ἐλάσαντε]ς
ζαθέας τροφοῦ· ἀλλὰ [βαρεῖα μέν
ἐπέπεσε μοῖρα· τλάντ[ω]ν

[38] See Ch. Koukouli-Chrysanthaki, *Ergon* 1982, 11f.: 1983, 16.
[39] Ch. Koukouli-Chrysanthaki, *Ergon* 1984, 12f.; *cf. Arch. Rep. for 1985-6*, 70f.

δ' ἔπειτα θεοὶ συνετέλεσσα[ν.
ὁ δὲ καλόν τι πονή[σ]αις
  εὐαγορίαισι φλέγει·
κείνοις δ' ὑπέρτατον ἦλθε φέγγος
ἄντα δ[υ]σμενέων Μελαμ-
  φύλλου προπάροιθεν.

As Radt rightly states (p. 59), the supplement 'Α[θόω] is made certain and [λαοὺς ἐλάσαντε]ς is at least made probable by the beginning of the scholion to line 63: [ὑ]π[ὲ]ρ [τὸ]ν Ἄθω ἐκβληθέντες | οἱ ἐνοικο[ῦ]ντ[ε]ς ἐπῆλθον | ⟨ἀ⟩μυνούμ[ενοι το]ὺς ἐκβαλόν|τας καὶ ἐνί[κησ]αν. A translation (which owes much to Sandys' in the Loeb and to Radt's on pp. 22f.) of the passage follows: "They who won by war the richly-dowering land established prosperity, after they had driven away beyond Athos from their most holy foster-mother the troops of Paeonian spearmen. But a hard fate befell them; yet, because they endured it, the gods later helped to complete their task. And he who has by toil achieved a noble deed shines with praises. For them the supreme light came face to face with the foe before Melamphyllon."

The fighting here referred to had three phases. In the first we are told that the colonists[40] drove Paeonians beyond Mount Athos. The identity of the enemy and the topographical indication have both provoked discussion.

The Paeonians were thought to be Pindar's mistake by Wilamowitz,[41] and he has been followed by some later scholars, but it has been rightly recognized that Pindar and his hearers would not have accepted a basic error of fact in quite recent history.[42] Moreover, the Paeonians fit very well as the local opponents here. At about the same time the Paeonians were able to traverse southern Thrace in order to attack Perinthus.[43] They can be seen as the most powerful people in the area from *c.* 550 until the Persian conquest of Thrace.[44] It is

There is also archaeological confirmation for the date of the foundation of Phanagoria; see J. G. F. Hind, *Arch. Rep. for 1983-4*, 90.

[40] Radt (57f.) effectively refutes the idea that there is a reference to the Clazomenian colonists here.

[41] *Sappho und Simonides*, 250.

[42] *Cf.* Radt, 60.

[43] Hdt. v 1; *cf.* Radt. *ibid.* and Hammond's remarks in N. G. L. Hammond and G. T. Griffith, *History of Macedonia* ii (Oxford 1979) 55-7.

[44] Hammond, *ibid.*, who cites, in addition to Herodotus, Strabo vii frg. 41, but, regrettably, not this passage of Pindar.

particularly important to note that the Paeonians controlled the land as far as Pangaeum,[45] where there were famous deposits of precious metals. Radt acutely remarked (58) that Pindar used an unusual epithet for the land (χθόνα) in πολύδωρον (line 60), an almost exclusively Homeric || word, employed to describe women bringing rich dowries, because he meant to remind his hearers that the land was not only fertile but also contained precious metals. This suggestion may be accepted with confidence, because we can see from the early coinage of Abdera, which contains very large denominations, that the colony was from the beginning exporting precious metal in the form of coins.[46] So there is nothing surprising in the Teian colonists being strongly opposed by the Paeonians.

It has also been thought that the phrase 'beyond Athos' cannot be taken literally, since the Teians cannot have pushed back the Paeonians so far, but should rather be seen as poetic hyperbole for 'beyond the horizon'.[47] However, if we have a correct conception of Paeonian power, and of the value of the prizes at stake, then the distances involved become entirely intelligible. Although our picture of the first stage of fighting is inevitably vague and without detail, we nevertheless have in general a very striking example of the use of force and scale of warfare required to establish a colony in Thrace.

The initial victory of the Teian colonists was followed by a defeat, which Pindar refers to in very general terms (63f.): ἀλλὰ [βαρεῖα μὲν] ἐπέπεσε μοῖρα, a passage which is difficult to restore in detail.[48] Fortunately, however, the scholion quoted above makes it clear that the colonists of Abdera suffered a defeat at the hands of the native inhabitants. Because the colonists endured the setback, the gods helped them to final success (64f.): τλάντ[ω]ν δ' ἔπειτα θεοὶ συντέλεσσα[ν]. Pindar's gnome about the shining glory for those who face trouble nobly, which follows (66f.), looks back at this endurance and forward to the highest light gained by those who fought the enemy at Melamphyllon (68-70).

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[45] Strabo, *ibid.*

[46] See J. M. F. May, *The coinage of Abdera* (London 1966) especially 1-17. I gave reasons for rejecting the more recent downdating of the early coins of Abdera in my 'Adopted Teians etc.' (see n. 4) n. 18.

[47] Even Radt mistakenly follows the earlier commentators here (60).

[48] See Radt, 62f.

[49] Wilamowitz' perverse idea that the battle at Melamphyllon was a defeat for

So these two stages are a defeat bravely borne followed by the final victory at Melamphyllon.[49] We know from Pliny (*N.H.* iv 50) that there was a mountain in Thrace called Melamphyllos, and the scholiast to line 70 says that Melamphyllon was a place in Abderan territory, but, as before, we have no precision or detail about the engagement.

There is also no indication in Pindar of the chronology of these operations. However, since it is inconceivable that they could have taken place after the Persian satrapy of Thrace was created (and, *a fortiori*, the immediate subsequent Persian deportation of the Paeonians),[50] the widest time-within-which must lie between *c.* 545 and the date of that Persian conquest. The latter date is, unfortunately, connected to the date of Darius' Scythian Expedition, one of the worst chronological *cruces* of the period, but which cannot be earlier than *c.* 520 or later than *c.* 512.[51] So the latest possible date for the creation of the Persian satrapy is very shortly after || *c.* 512. We know that Darius could reward Histiaeus for his loyal service in the Scythian Expedition with the gift of Myrcinus, which was then his territory.[52] These considerations show that the time available for the military operations under discussion was, at the longest, a little more than thirty years. They belong, therefore, to the first generation of Teian Abdera, and should be seen as part of the history of its foundation.

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the Abderites is easily refuted. See Radt, 61f., whose fine and convincing treatment of lines 63-70 I have followed closely. This establishes the three stages noted. Isaac, however, *op. cit.* (n. 4) 86, still follows Wilamowitz.

50 For the creation of the Thracian satrapy, see Hdt. v 1.1; 2 (*cf.* iv 143-4). There are very useful modern discussions by N. G. L. Hammond. "The extent of Persian occupation in Thrace," *Chiron* x (1980) 53-61, and W. Pajadowsky, "Einige Bemerkungen zur Lokalisierung der persischen Provinz (Satrapie) Skudra," *Eos* lxxi (1983) 243-55, who wrote before he had read Hammond (255 n. 56). For the deportation of the Paeonians, see Hdt. v 12-14.1; *cf.* Hammond, 56-8, who dates it *c.* 512-10.

51 See J. M. Balcer, "The date of Hdt. IV.1" *HSCP* lxxvi (1972) 99-132, a detailed argument for a high date, 519, which the author has since withdrawn; see *Sparda by the Bitter Sea* (Chico CA 1984) 474 n. 69. The only recorded date from Antiquity, 513/12, is preserved in the so-called *Chronicon Romanum* (*IG* xiv 1297 = *FGH* 252) B(8) and monuments derived from it; see now S. M. Burstein, "A New *Tabula Iliaca*: the Vasek Polak Chronicle," *The Paul J. Getty Museum Journal* xii (1984) 153-62. J. R. Gardiner-Garden's survey of the problem ("Dareios' Scythian Expedition and its aftermath," *Klio* lxix [1987] 326-50, at 326-30) makes abundant reference to modern scholarship, but does not mention Balcer's palinode nor Burstein.

52 Hdt. v. 11.2; *cf.* Hammond, 59.

The city they colonized is situated on a substantial headland, bounded on the south and east by the sea (Figs. 11, 12). To the West there are marshy swamps close at hand, and there are also lakes and swamps up the east coast to the north. Due north there are rolling hills, where the very extensive city cemeteries were placed. The city is basically low-lying, with a moderate || acropolis hill in the south-west corner overlooking the main (west) harbour (see Figs. 11, 12).[53] While the cemeteries show that the general position of the city was always the same, we cannot tell its extent in the early Teian (much less the Clazomenian) period. A wall circuit of over five kilometres has been partly uncovered but is mostly presumed (Fig. 12). It is obviously also mostly undated, but is thought to belong to the Classical and later city. More recently another circuit to the north has been revealed, which, it has been suggested, defended the early Teian, or even the Clazomenian, city (Fig. 13).[54]

Lazarides suggested that the *chora* of Abdera was bounded to the south by the sea, west by the River Nestos, north by the foothills of Mount Rhodope, and to the east by Lake Bistonis (Fig. 11).[55] It is perfectly reasonable to suggest that the very big city had a very large *chora*, but we do not know when such boundaries were achieved. Our only clear chronological indication seems to be Herodotus' information that, when he wrote, the Nestos ran through || Abderan territory (vii 126).[56] Since the river is *c.* 16 km. in a direct line west of the city, a large *chora* may by that time be assumed.

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[53] D. Lazarides, Ἄβδηρα καὶ Δίκαια (Athens 1971), with its many maps and plans, is still very useful for the topography, even if now outdated archaeologically. The accounts of Skarlatidou and Koukouli-Chrysanthaki (nn. 29, 30) make use of the more recent discoveries.

[54] See Koukouli-Chrysanthaki, 83f., but see also above n. 30.

[55] *Op. cit.* p. 2. These limits are also accepted by Koukouli-Chrysanthaki, 84f., 89. She also follows Lazarides in arguing that the early *chora* certainly extended at least *c.* 7 km. to the North of the city, because the colonists were using the limestone quarries in the vicinity of the modern villages of Mandra and Abdera by the end of the sixth century.

[56] That this is the correct way to take Herodotus' words, ὅ τε δι' Ἀβδήρων ῥέων ποταμὸς Νέστος, was recognized long ago. See, e.g., How and Wells, *Commentary* ad loc., who compare Herodotus' use of ἐς Μιλητόν in i 15, which certainly refers to the territory only. Note also Pausanias' explicit statement that the Nestos runs through the land of the Abderites (vi 5.4). Thus Isaac is seriously astray in stating that the river "passed by the walls of Abdera:" *op. cit.* (n. 4) 73.

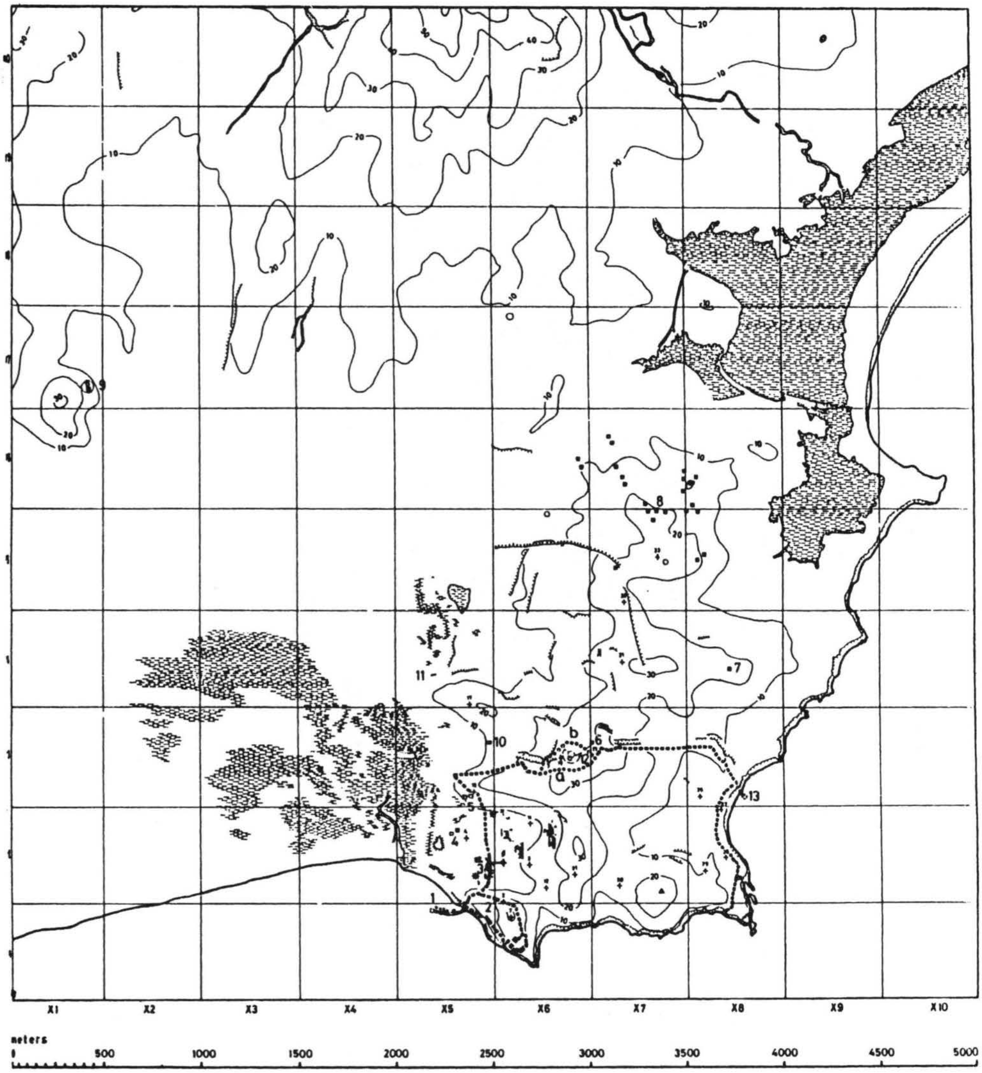

Figure 12. Abdera: site and presumed wall circuit of Classical period (after Lazarides, *Abdera and Dikaia*, fig.28)

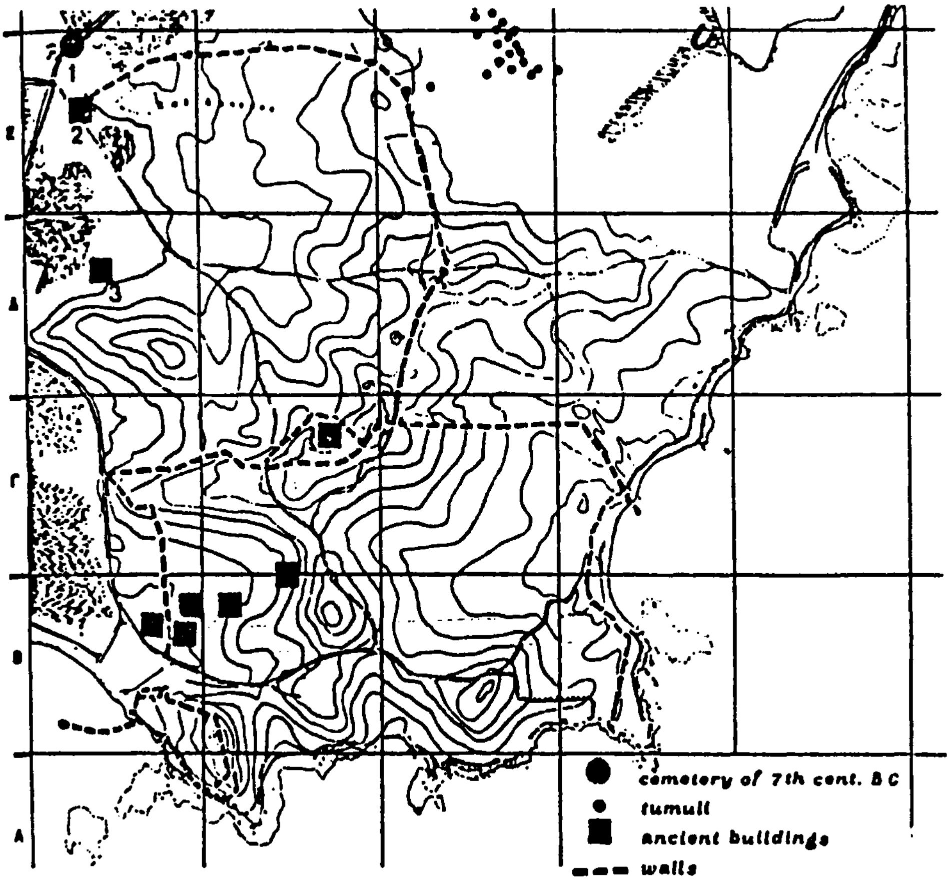

Fig. 13. Abdera: site showing newly discovered North wall circuit and oldest cemetery (nr. 1) (after Koukouli-Chrysanthaki, *Thracia Pontica* 3, fig. 1).

We have little evidence for the resources of the city in the early period, down to the Persian Wars. Herodotus tells us that Abdera found it a great strain to provide dinner for Xerxes and his followers (vii 120; viii 120), but that story is rather anecdotal. The clear numismatic testimony that Abdera exported precious metal widely has led to the inference that the city was prosperous from the beginning.[57] Such assumptions may well be justifiable but they are not quantifiable. Later on we have much better evidence in the Athenian Tribute Lists, but they take us too far from the time of foundation.

[57] See May, *Coinage of Abdera*, 1-4.

The Refoundation of Teos

Before the allusions to the military engagements discussed in the last section, Pindar's Second Paean contained references to other historical events. Unfortunately, the state of the text and scholia for lines 37-55 is too uncertain for them to be of use to a historian,[58] and the only historical event that is now clear to us is Abdera's refoundation of its mother city, Teos (28-34). I have discussed this recently elsewhere,[59] which absolves me from the need to repeat the arguments here. We can confidently accept that at some time in its early history Teian Abdera sent sufficient settlers back to her mother city for this to count as a refoundation. It cannot be certainly established when Abdera refounded Teos. The two most probable occasions are shortly after the evacuation of the city in *c.* 545 and shortly after the Ionian Revolt. In any case, we know that Teos was repopulated in one way or another fairly soon after its evacuation, since it existed again as a moderate-sized city by the time of the Ionian Revolt.

That there were close and good relations between Abdera and this revived Teos quite soon after the foundation of the colony is proved by the coinage of the two cities. The first coins of Abdera and the first silver coins of Teos are so closely alike that it seems probable that these coinages "were started in planned conjunction."[60] These coins and the refoundation of Teos by Abdera provide the earliest evidence that there were very close relations between the colony and its mother city.

The Public Imprecations of Teos

The next evidence on this topic comes from one of the inscriptions from Teos which record public imprecations. We know of two Teian

[58] The largely complete lines 50-58 and the tantalizing political allusions in the corrupt scholion to line 48 tempt speculation, but Radt's careful discussion (49-57) of the possibilities and the suggestions that have been made shows that we are reduced to mere guesswork. Even his modest conclusion (51) that lines 48-50 certainly refer to internal strife in Abdera, in which some immigrants played a part, may be a little optimistic.

[59] See "Adopted Teians etc." (n. 4).

[60] See C. M. Kraay, *Archaic and Classical Greek coins* (London 1976) 35, and *cf.* my "Adopted Teians etc."

inscriptions that recorded these curses on malefactors, which magistrates were required to pronounce on public occasions at regular intervals. The first, ML 30, has been known for a long time, though we are dependent on the copies of early travellers, as the stone is long lost.[61] The second, *SEG* xxxi (1981) 985 is a recent discovery, and has been well read and published.[62]

These inscriptions cannot be dated very closely epigraphically. The recently discovered one, where we at least have the stone, has been judged by Herrmann, on the basis of letter forms, to belong to the period *c.* 480-450, preferably nearer to the lower of these dates.[63] ML 30 was long ago dated *c.* 470, and this date has been generally followed.[64] Apart from epigraphical criteria, the ban against setting up an *aisymnetes* in both inscriptions makes it likely that they belong to the period after, but not too long after, the Ionian Revolt and Persian War period.[65]

There are two features of *SEG* xxxi, 985 which differentiate it from ML 30: firstly, part of it is couched in the form of a citizen's oath (A.10-24),[66] though the subject matter is very similar to the rest of the document; and, secondly, there are references to Abdera as well as Teos. Since these references are of vital importance for the light they throw on the relations of the two cities, they are examined in turn here.

A. 13-22 ο[ὐ]δ̣ὲ̣ διώξω: ο-
[ὐ]δ̣ὲ [χρ]ή̣μ[α]τ̣α: δημιώσ-
[ω: οὐ]δὲ δήσω· οὐδὲ κατ̣-
[ακ]τ̣ε̣[ν]έ̣ω: ἂμ μ̣ὴ̣ σ[ὺ]ν..
[. . .]Ι̣.Ι̣ΣΙΝ̣: ἐν̣ Τ̣έωι
[ἢ] π̣λέο̣σ[ι]ν: [κ]α̣ὶ ἂμ μ̣ὴ̣ ὑ̣-
π[ὸ] πόλεω[ς]: ν̣[ό]μο̣: κατα-
λαφθέν[τ]α̣: ἐν δὲ Ἀβδή-
[ρ]οισιν: [σ]ὺμ πε̣ντακο̣-
[σ]ί̣οισιν: ἢ πλ[έο]σιν:

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[61] See Herrmann (n. 1) 1-2, for the epigraphical history of the inscription, and *SEG* xxxi (1981) 984 for the improvements in the readings of the text resulting from the discovery of the new one. See also McCabe and Plunkett (n. 1) 261.

[62] See n. 1. I refer to Herrmann's paper hereafter by the author's name alone.

[63] Herrmann, 6.

[64] Herrmann, 3 with n. 10. On the dating *cf.* e.g., ML 30 and *SEG* xxxi 984.

[65] See my "Adopted Teians etc." (n. 4).

[66] See Herrmann's enlightening discussion, 13f.

Translation:

I shall not prosecute, nor shall I confiscate property, nor shall I arrest, nor shall I put to death, unless with 200 (?) or more, in Teos, and unless he is condemned by a law of the city, and in Abdera with 500 or more.

B. 5-12 τοῦτον
[ἀ]πόλλυσθα-
ι: ἐκ Τέω: κ[α]ὶ
Ἀβδήρ[ω]ν: [κ]α-
ὶ γῆς: [Τηΐ]η[ς]
καὶ α[ὐ]τὸν κ-
αὶ γένο[ς] τὸ
κείνο:

Translation:

May he be destroyed (and banished) from Teos and Abdera and the land of Teos, both himself and his family. ||

C. [. . . . Ἀβδ]ηριτέων: ἀ[π]αιτέο[ν]-
[τος τô ξ]υνô: μὴ 'π[ο]διδ[ο]ίη: κεῖ[ν]-
[ον ἀπ]όλλυσθαι: καὶ αὐτὸν: κ[αὶ]
[γ]ένος τὸ κένο

Translation:

. . . if the community of the Abderites request (it) back, should he not give (it) back, may he be destroyed, both himself and his family.

D. 1-23 Ἀνθ[εστη]ρ[ί]-
οισιν: καὶ Ἡ-
ρακλέοισι-
ν: καὶ {ι} Δίοι-
σιν: ἐν Ἀβδ[ή]
ρο[ι]σιν: Ἀν[θ]-
εστηρίοι[σ]-
ιν: καὶ Ἡρα[κ]-
λέοισιν: κ[α]-
ὶ Ζηνὸς: ἑορ-
τῆι: Ὅστις δ-
ὲ τιμοχέων
ἢ ταμιεύων
μὴ 'ναλέξεε-
ν: τὰ γεγραθ-
μένα: ἐν τῆι
[σ]τήληι: ἐπὶ
μνήμηι: καὶ
δυνάμει: ἢ [φ]-

οινικογρα-
φέων: κελευ-
[ό]ντων τιμό-
 χων: κεῖνον

Translation:

> ... at the festival of the Anthesteria, the Heraclea and the Dia, in Abdera at the Anthesteria, the Heraclea and the festival of Zeus. Whoever in the capacity of a *timouchos* or treasurer does not read out the things written on the stele for the purpose of reminder and validation(?),[67] or whoever as secretary on the order of the *timouchoi* (fails to do so), may he ...

In the first of these passages a resolution about criminal justice in the citizen's oath is found to apply also to Abdera, and the quorum for serious criminal decisions is fixed for Abdera as well as Teos.[68] In the second a malefactor is declared an outlaw from Teos, Abdera and the land of Teos. The incompleteness of the third passage and inevitable uncertainty of restorations leave its significance more obscure. As restored, if someone does not give back something requested back by the community (τὸ ξυνόν) of the Abderites, he is to be destroyed. As Herrmann noted (23), the use of the term ξυνόν of Abdera is not paralleled, but it occurs more than once in relation to Teos in ML 30.[69] He also pertinently raised the question whether || Abdera was alone the concern of this provision, or was, as elsewhere, also linked with Teos here. In the final passage the document provides for the reading out of its provisions at public festivals at both Teos and Abdera, and imposes the normal penalty on the competent officers in the case of failure. The only difference between the two cities is that the festival called the Dia at Teos is called the Festival of Zeus at Abdera.

Before we consider the political implications of these passages, it is necessary to cite a section from the early Hellenistic decree establishing *sympoliteia* between Teos and Kyrbissos, *SEG* xxvi (1976)

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[67] For the difficulties of interpretation here, see Herrmann, 11-12. I follow the suggestion of Wörrle reported in Herrmann, n. 29.

[68] For the number of the quorum at Teos, probably 200, see D. M. Lewis, "On the new text of Teos," *ZPE* xxxxvii (1982) 71f., who adduces illuminating parallels and stresses the democratic significance of these relatively large bodies.

[69] A.3, B.3 (if we accept the new texts proposed by Herrmann and Merkelbach; see *SEG* xxxi 984), B.25.

1306 (= McCabe and Plunkett, *Teos inscriptions* 48) 20-26, which, as Herrmann saw (22f.), is very closely analogous to the material under discussion.

> ὃς δ’ ἂν παραλαβὼν |[22] τὸ χωρίον μὴ παραδῶ[ι τ]ῶι φρουράρχω[ι] τῶ[ι] ὑπὸ τῆς πόλεως ἀποσ[τελ]|λομένωι ἀεὶ καθ’ ἑκάστην τετράμη[νο]ν, φ[ε]ύγειν τε αὐτὸν ἀραιὸν | ἐκ Τέω καὶ ἐξ ’Αβδήρων καὶ ἐκ τῆς χώρας καὶ τῆς Τηΐων καὶ τῆς ’Αβδηρ[ι]|τῶν καὶ τὰ ὄντα αὐτοῦ δη[μό]σια ε[ἶ]ναι, καὶ ὅς ἂν ἀποκτείνηι αὐτὸν μ[ὴ]|[26] μιαρὸς ἔστω·

Translation:

> Whoever, having taken over the place, does not hand it over to the Garrison Commander sent by the city, on each occasion, every four-month period, he shall be exiled and accursed from Teos and Abdera and from the land both of the Teians and of the Abderites, and his possessions shall be confiscated, and whoever kills him shall not be polluted.

In his useful discussion (26-30) of the significance of this evidence for the relations of Abdera and Teos, Herrmann is understandably cautious about drawing definite or precise constitutional conclusions from the limited and in some cases uncertain evidence.[70] However, he rightly accepts that the relationship we see must be the result of Abdera's and Teos' situation as colony and mother city, especially as the Teian colonization of Abdera is so close in time to the composition of the document under discussion. Thus anyone interested in the relations of Greek colonies and mother cities is bound to try to assess the relationship attested by this new evidence.

To begin by simple and general considerations, we have here a law of Teos which legislates for Abdera. When a Greek city legislates for its colony(ies), one obvious explanation would be that the mother city has hegemony. An example of that would be Thasos' hegemony over its mainland colonies, strikingly displayed in the inscription ML 83.[71] The hegemony of Teos over Abdera seems, however, very improbable. Abdera was much larger than Teos,[72] and it was the refounder of Teos, to mention only the most obvious objections.

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[70] Herrmann is closely followed by Huxley in his short discussion, *Studies . . . Dow*, 151.

[71] See my discussion, *Colony and mother city in ancient Greece*, 2nd ed. (Chicago 1983) 83-5.

[72] As Lewis pointed out (see n. 68), the quorums established for serious criminal

The other immediate possible explanation is that we have something like *sympoliteia*, whereby the two cities formed a single political entity.[73] Apart from the general fact that Teos legislates for Abdera, Herrmann thought (28) that the mention of 'a Law of the city' only for Teos (A.19) and of only 'the land of Teos' in the banishment clause (B.9) point in that direction. It does not seem to me absolutely certain that his understanding of these two passages is inescapable. In the first 'a law of the city' could, it seems to me, perhaps refer to a law of Abdera as well as a law of Teos. And the banning clause is so closely paralleled by the passage in the decree about *sympoliteia* with Kyrbissos, where the land of Abdera is expressly specified, that it may be possible that the earlier document was simply abbreviating, and Abdera stood for || both city and land, rather than implying that Abdera's land was a Teian possession. However, even if these two passages can perhaps be explained away, we still have the fundamental fact that Teos legislates for Abdera in all the four passages cited, and the same is true of the decree about *sympoliteia* with Kyrbissos.

One way to try to define the phenomenon here attested might be to categorize the areas of public life in which Abdera and Teos seem to be closely linked. Thus Herrmann (26), following the lead of J. and L. Robert,[74] noted that the passage from the *sympoliteia* decree provides the proof of "einer Zusammengehörigkeit . . . in einem besonderen juristisch-religiösen Bereich."

If we attempt this, we find first that the constitutional and religious arrangements, and the public officials and their titles (so far as all this is attested) are, apart from details,[75] the same in both communities. That is, in itself, not surprising,[76] and need not reflect (though it could) any merging of political existence. In detail, the provisions show Teos establishing parts of a citizen's oath at Abdera in connection with serious criminal justice, arranging the procedure

justice in the two cities are in the same proportion, 5:2, as the tribute they regularly paid in the Athenian Empire, 15 and 6T.

[73] Admitted tentatively as a possibility by N. Ehrhardt, *Milet und seine Kolonien* (Frankfurt 1983) 234.

[74] *Journ. des Savants* (1976) 212f.

[75] The size of quorum and the name of the Festival of Zeus.

[76] See my *Colony and mother city*, 14f. and now the very useful work by Ehrhardt, *op. cit.* (n. 73).

for such justice, causing an outlaw from Teos to be also an outlaw from Abdera, and laying down the procedure for proclaiming the imprecations at Abdera at religious festivals and imposing penalties for failure to do so. (I omit the passage from face C as too uncertain in significance.) It may be formally possible to categorize these acts as belonging to the sphere of religion and justice, but, if one considers the very great importance of justice in any Greek city's constitution, and the overriding and all-embracing role of religion, it would be artificial to regard such a categorization as a limitation. We cannot evade the facts that Teos legislates for Abdera in general and the legislation concerns some of the most important aspects of the public life of a Greek city.

This evidence seems, therefore, to point unquestionably to some merging of the political identities of Abdera and Teos. Herrmann (28) also adduced Herodotus' description of the Abderites—referring to his own day—as Τηΐων τῶν ἐν Ἀβδήροισι (i 168), and asked if Herodotus had a political situation in mind, when he called the Abderites 'Teians in Abdera'. At about the same time Protagoras of Abdera was called a Teian by Eupolis, a contemporary comic poet.[77] But this evidence can hardly be pressed for political and legal conclusions. There are many examples which show that Greeks could use ethnics without caring about legal precision.[78] Moreover, at i 168 Herodotus has a reason for stressing the Teian origin of the colonists: he is contrasting them with their Clazomenian predecessors and the Clazomenian origin of the *oikistes* whom they honoured. Where he has no such ideas in mind, he is quite able to use the ethnic Ἀβδηρίτης (vii 120; viii 120).

So the evidence from the use of ethnics cannot be confidently added to the indications of some merging of political identity by Abdera and Teos. Is there contrary evidence of political separateness? Here Herrmann (28) mentioned the obvious possession by each city of a separate assembly of the people and the possibility that an individual community (ξυνόν, C.1-2) of the Abderites could pass its own decrees and express its own will.

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[77] Eupolis fr. 157 Kassel-Austin; *cf.* Diog. Laert. ix 50.
[78] I collected some in *Colony and mother city*, 103-5.

These points are not decisively probative, however, because there is an element of uncertainty in the restored ξ]υνô,[79] and the existence of an assembly at Abdera is not explicitly attested for the period in question. There is no real doubt that we should assume its || existence, and the inscription does show us separate courts for criminal justice at Abdera as well as separate celebrations of religious festivals. All the institutions of a Greek city-state would have been required there from the beginning, but it could be pointed out, if a little pedantically, that these institutions were present in Athenian cleruchies, which did not have a separate political existence.[80] Ideally we need other evidence.

Each city paid tribute separately in the Athenian Empire,[81] and they should be seen as essentially separate entities at that time. However, we know of examples of colonies of Thasos, Neapolis and Galepsus, which paid tribute separately but were nevertheless subject to the hegemony of their mother city.[82] Even though it was seen to be most improbable that Abdera was under the hegemony of Teos, these examples show that separate paying of tribute does not establish perfectly independent existence as a separate state.

Abdera and Teos also each issued its own coinage, which has often been taken as a sign of independent statehood. However, it can no longer be maintained that there is an exact correspondence between issuing coins and political status,[83] so this argument is not decisive. It may be that we cannot find in the evidence now available to us an absolutely irrefragable proof of Abdera's separate political existence in this period. On the other hand, it does seem an offence against common sense to imagine that the large and well-known Greek city, which successfully maintained itself on the coast of Thrace, was not politically independent and able to take its own political decisions from the beginning (apart, no doubt, from its time

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[79] See above p. 55.

[80] I discuss Athenian cleruchies in *Colony and mother city*, 167-92. This is not the place to enter into the still lively arguments about their nature and purpose; see T. J. Figueira. *Athens and Aigina in the age of imperial colonization* (Baltimore 1991), especially 40-73, with full bibliography.

[81] ATL i, Register s.v.v.

[82] See my *Colony and mother city*, 83-90.

[83] The issue has been well argued by T. R. Martin, *Sovereignty and coinage in Classical Greece* (Princeton 1985); *cf.* also my own brief treatment, *Colony and mother city*, 121-8.

as a vassal of Persia). If that is right, our indications point in two directions: one is some merging of political identity of Abdera and Teos, and the other is the separate political existence of both cities.

In these circumstances, it is hardly surprising that Herrmann concluded (29): "Man sieht: auch wenn man es von den Entstehungsbedingungen her für plausibel hält, dass in diesem besonderen Falle die Mutterstadt und ihre Kolonie von einem Grundverständnis politischer Zusammengehörigkeit und staatlicher Einheit ausgingen, führt doch die Frage nach der organisatorischen Form der Aufrechterhaltung dieser Einheit in unserer Vorstellung zu Schwierigkeiten." The next recourse must obviously be to comparisons.

The analogy adduced by Herrmann (*ibid.*) was Potidaea, to which the mother city, Corinth, annually sent magistrates, the *epidemiourgoi*.[84] However, one of the reasons he cites this case is to show how an isolated piece of evidence on such a topic leads to differences of interpretation. For help in understanding Abdera's relations with Teos it is not an ideal comparison. There is too much evidence suggesting the primacy, or even hegemony, of Corinth over her colonies for us to choose a different line of interpretation in the case of Potidaea.[85]

If we admit hegemonial relationships, there is a rather good parallel for the banning clauses in the famous Rhodian athlete Dorieus, who is described by Xenophon (*Hell.* i 5.19) as a fugitive or exile (φυγάδα) from Athens and Rhodes, having been condemned to death by the || Athenians together with his relations.[86] The condemnation by the imperial hegemon naturally extended over the territory of the subject ally. No doubt the Athenian decision that serious criminal cases arising in the allied states be tried at Athens led to many such instances.[87] But there is no real similarity between Athens' relations with her allies and those of Teos with Abdera. We should seek rather *comparanda* where hegemony is not likely.

The most enlightening example known to me is the relationship of Paros and Thasos, which was already adduced by J. and L. Robert, when they discussed the banning clause in the decree

[84] Thuc. i 56.2.

[85] I discuss Corinth's relations with her colonies in *Colony and mother city*, chapter 7, and the *epidemiourgoi* at Potidaea at 136f. On the latter no advance seems to be offered by J. B. Salmon's treatment, *Wealthy Corinth* (Oxford 1984) 392-4.

[86] On Dorieus see Meiggs, *Athenian Empire*, 368f., with further ancient evidence.

[87] Athens' imperial jurisdiction is discussed in general by Meiggs, 220-33.

about *sympoliteia* with Kyrbissos.[88] At about the end of the sixth century Akeratos boasted that he had been a magistrate at both Thasos and Paros,[89] and in *c.* 340 the Athenian general Cephisophon was honoured in a decree of Paros "because he is a benefactor of the people of the Parians and Thasians."[90] The very close relations between Paros and Thasos which are attested by this evidence seemed to one commentator to be an example of *sympoliteia.*[91] Arguments can be offered against that conclusion,[92] but we undoubtedly have a far closer relationship than would be normal between two independent states, one where "the legal division between the two states was slightly blurred."[93]

This analogy seems apt for the relations of Abdera and Teos, not only chronologically, but, more importantly, because there is no doubt that Paros and Thasos were separate states, and we can dismiss any idea that Paros exercised political hegemony over Thasos. In both cases we find a Greek colony and mother city which were independent states, but were so closely linked together that they admitted institutions and arrangements that effectively placed their political unity above their political independence.

The Resources of Abdera

One of the incidental pieces of information from the new Public Imprecations inscription is that, if we judge from the quorums, Abdera was probably two and a half times as big as Teos. As we have seen, the same result is achieved by comparing the tribute each paid in the Athenian Empire.[94] We considered briefly early indications of the resources of Abdera above;[95] it is appropriate here to bring in more evidence by extending the timescale.

[88] *Journ. des Savants* 1976, 213.

[89] *IG* xii Suppl. 412; see my discussion in *Colony and mother city*, 74-6.

[90] *IG* xii.5.114, which I discuss *ibid.*, 79-81.

[91] See L. Robert, 'Inscription hellénistique de Dalmatie', *BCH* lix (1935) 489-513, at 500 (= *Opera minora selecta* i 302-26, at 313); cf. *Journ. des Savants* (1976) 212f.

[92] As they were by me, *ibid.* However, had the evidence about Abdera and Teos been available at that time, I might have written differently.

[93] *Colony and mother city*, 97.

[94] See nn. 68, 72.

[95] See above, p. 53.

The best evidence for the period after the Persian Wars comes from the Athenian Tribute Lists. Abdera pays regularly 15T from the time when our records begin till 432/1, when she paid 10T, a figure also attested in one subsequent list.[96] Strikingly out of line with this evidence is the proposal that Abdera and Dicaea by Abdera, which pays most commonly 1/2T, were assessed to pay together 75T in the Great Re-assessment of 425, commonly known from || the terminology of *ATL* as A9.[97] At the top of the Thracian panel there are preserved two very high assessments, 75 and 60T, but, unfortunately, the names of the payers are missing. Here is how column III lines 152-5 were restored by A. B. West and B. Meritt,[98] a restoration taken over by *ATL* and repeated, much more recently, in *IG* i³ 71:—

152		[Θράικιος φόρος]
153	𐅊𐅉𐅉[𐅈]	[hαβδερῖται]
154		[Δίκαια παρὰ Ἄβδερα]
155	𐅊𐅉	[Θάσιοι]

The argument for the massive and audacious supplement of lines 153-4 is twofold:[99] (1) [Δίκαια παρ' Ἄβδερ]α was restored in column IV line 30 of the next re-assessment, A10 (= *IG* i³ 77), because the alpha which can be made out at the break to the left of column V line 30 must come from a long name in column IV, and, if [Δίκαια παρ' Ἄβδερ]α is accordingly restored there, it must have stood, because of the reconstructions higher up in column IV, at or near the top of the Thracian panel; (2) the assessment figure in A9 column III line 153 is so placed that it must have been "associated with an entry of two lines in the column of names." These two arguments support each other because the appearance of the insignificant Dicaea by Abdera so near the top of the Thracian panel in A10 means "in all probability, that it was associated in assessment with its powerful neighbour, Abdera."

[96] *ATL* i Register s.v. 'List 25' of *ATL* (= *IG* i³ 281), attributed to 430/29, cannot be certainly dated; *cf.* Meiggs' discussion, *Athenian empire*, 531-7.

[97] *ATL* ii A9 = *IG* i³ 71 = ML 69 (selections).

[98] *The Athenian assessment of 425* BC (*University of Michigan Studies, Humanistic Series* xxxiii; Ann Arbor 1934) 80.

[99] *Ibid.*

There are, however, also strong arguments against the restoration: (1) Abdera and Dicaea never pay in a syntely in any extant list: (2) the figure of 70+T is a far bigger multiple, at nearly five times, of Abdera's and Dicaea's largest combined tribute of earlier years. 15 1/2T, than is generally found even in the optimistic assessments of A9.

Nor are the purely epigraphical arguments completely compelling. It is possible with the eye of faith to see parts at least of the vital alpha at A10 column IV line 30,[100] but West himself once made a different suggestion for the restoration of this line.[101] And while it is true that the assessment figure in A9 column III line 153[102] is more spaced from its successor than most, it is less spaced than the figure of a certain two-line entry from the previous column on the same fragment, column II lines 151-2:

H 'Ελαιόσιοι
'Ερυθραίων,

where the H is further from the figure above than our figure is from the figure below.[103]

There is certainly a problem in the two very high assessments at the top of the Thracian panel in A9. They are far the highest preserved in all the extant parts of the inscription; the next highest is 30T for Paros; and in the Thracian district only Thasos, which had earlier paid 30T,[104] seems a likely candidate for such high assessments. Even so, there are too many uncertainties and too strong objections to the suggested syntely of Abdera and Dicaea for it to be regarded as necessarily the correct solution. For our purposes, therefore, it seems prudent to || use only the figures of actual tribute paid by Abdera.[105] The highest figure, 15T, would suggest that she was half as rich as Thasos and a little less rich than Byzantium, which paid 18T at the maximum.[106]

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[100] See *ATL* i 119, fig. 173.
[101] West and Meritt (n. 98) 80, n. 1.
[102] See *ATL* i 115, fig. 36.
[103] See *ibid.*
[104] *ATL* i Register s.v.
[105] Isaac accepts that Abdera and Dicaea actually paid 75T; *op. cit.* (n. 4) 98f.
[106] *ATL* i Register s.v. Isaac notes how few cities in the Athenian Empire paid more than 15T; *op. cit.* 94.

These calculations and comparisons would be complicated if Abdera was also paying to the Odrysian kingdom. Thucydides (ii 97.3) tells us that these kings received tribute from Greek cities as well as their non-Greek subjects, and that at its highest, under Seuthes I, who succeeded in 424 (Thuc. iv 101.5), the tribute paid by both totalled 400T in coined money, an equal sum in presents of gold and silver, not to mention presents in other materials. It is not known which Greek cities paid tribute to the Thracians. It was ingeniously suggested in *ATL* (iii 309f.) that we can use the treaty between Athens and the Thracian kings of 357, Tod ii 151, where the tribute paid by some Greek cities to the Thracians is called 'traditional', πάτριον (line 15), to conclude that such tribute "no doubt went back to the great days of Seuthes." On this basis the authors argued that the Greek cities of the Chersonese and "*a fortiori*... the coastal cites between Abdera and Byzantium" were liable to pay this tribute. Unfortunately, however, the inscription recording the treaty of 357 is very heavily restored, and, while it is clear that some Greek cities paid tribute to both Thracians and Athens, it is not certain which they were.[107] So it does not greatly advance our knowledge to bring in the fourth-century treaty, except that, by analogy, it might be held to show that at other times too Greek cities could have paid tribute both to Athens and to the Thracians.

Some have obviously thought that in the great days of her fifth-century empire Athens would not have tolerated such a situation. So Gomme suggested that Thucydides was 'most probably' referring to the Greek cities on the Pontic coast north of Mesembria.[108] But the argument is *a priori*, and there are no Greek cities on the coast of Thrace which were certainly always outside the Athenian Empire. On the other hand, there are arguments in favour of the assumption that Abdera paid tribute to the Odrysian kings at that time.

In the first place, when Thucydides gives the extent of the Odrysian kingdom (ii 97.1-2), his starting point for the length of its sea coast and for the shortest journey by land from south to north is the city of Abdera. Secondly, when he measures the distance by land from east to west, he begins from Byzantium. It is possible to argue in

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[107] Even the assumption that the cities were in the Chersonese depends on the completely restored name in lines 13f. The editors of *ATL* offered their own text with slightly different restorations; see *ATL* ii T 78D.

[108] *Commentary* on Thuc. ii 97.3.

both cases that he was measuring from the border of the territory of the Greek cities,[109] but, as the Greek goes, it is perfectly natural to understand that he is including Abdera and Byzantium in the Odrysian possessions.

Another argument arises from a possible explanation of the significant reduction in Abdera's tribute, from 15T to 10T, in 432/1.[110] In the summer of 431 Nymphodorus of Abdera arranged an alliance between Athens and Sitalces,[111] so it might be suggested that the reduction in Abdera's tribute to Athens represented some concession by Athens connected with Abdera's tribute to Sitalces. Obviously this is very hypothetical and other explanations are possible.[112] || But it is interesting, and may be significant, that there are large fluctuations in the tribute paid to Athens by other Greek cities on the coast of Thrace. Maronea paid 1 1/2T regularly from 454/3 to 441/0. Then there is no record till 436/5, when she paid 10T, as she does in the next two, probably three, years.[113] In 432/1 she probably paid 3T.[114] Selymbria's tribute, which had been 6 or 5T, appears in 435/4 as 900 dr.[115] The authors of *ATL* thought that these large fluctuations probably reflect changes in relations with the Odrysian kingdom, though they explicitly refrained from trying to interpret them in detail; and they were followed by Meiggs.[116]

So the possibility should be admitted that Abdera paid tribute to the Odrysian kingdom as well as to Athens. Even if she did not, she may well have had to give substantial presents.[117] In either case we should have to add an unknown amount to the 15T paid to Athens

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109 As Gomme, *Commentary* ad loc.

110 *ATL* i Register s.v.

111 Thuc. ii 29.

112 The reduction has been associated with a lower emission of coinage to suggest a fall in prosperity at Abdera; see May (though cautiously), *Coinage of Abdera* 143-6; Isaac, *op. cit.* (n. 4) 98f. Apart from the uncertainty of such a deduction, one weakness of such theories is the difficulty in precisely dating the coinage; *cf.* the different views of H. Mattingly, "The second Athenian Coinage Decree," *Klio* lix (1977) 83-100, at 92-5.

113 *ATL* i Register s.v.

114 I agree with Gomme, Commentary I, 212, that the editors of *ATL* were wrong to change the old restoration, HHH [Μαρονῖται] to HHH [Μεθοναῖοι] in list 23 (= *IG* i³ 280) Col. II line 67. The old restoration is well supported by their lists 25 and 26, and the "Absent from full panel" for Maronea is unique for the city and unexplained.

115 *ATL* i Register s.v.

116 *ATL* iii 309-13; Meiggs, *Athenian empire*, 249, 253.

117 As Isaac suggests, *op. cit.* (n. 4) 98.

as an index of Abderan prosperity. The comparison with Byzantium would still be valid, because Byzantium's relations with the Odrysian kingdom may be assumed to be similar to Abdera's,[118] but Abdera may have been more than half as rich as Thasos.

This discussion shows that Abdera was a rich and powerful city in the fifth century. Similar conclusions follow from consideration of the city's military potential. We know of two wars against local enemies which occurred later than the period of the colony's foundation. The first is alluded to, if, unfortunately, very obscurely, in Pindar's Second Paean, lines 73-9 and 102-6:

> ἀ]λλά μιν ποταμῶι σχεδὸν μολόντα φύρσει
> βαιοῖς σὺν ἔντεσιν
> ποτὶ πολὺν στρατόν· ἐν δὲ μηνὸς
> πρῶτον τύχεν ἆμαρ·
> ἄγγελλε δὲ φοινικόπεζα λόγον παρθένος
> εὐμενὴς Ἑκάτα
> τὸν ἐθέλοντα γενέσθαι.

> ἐμο[ὶ δ'ὦ ἐπ]έ[ω]ν ἐσ-
> λῶν ε]ὐκλέα[- -]ν χά[ρ]ιν,
> Ἄβδ]ηρε, καὶ στ[ρατὸν] ἱπποχάρμαν
> σᾶι] βίαι πολέ[μ]ωι τελευ-
> ταί]ωι προβι[β]άζοις.

Translations:

> (73-9) But near a river will he butcher him, when he has come with few arms against a numerous host. It befell on the first day of the month; and the red-footed maiden, gracious Hecate, announced the saying that will come to pass.

(102-6) But to me, Abderos (?),[119] give thanks for good words of fair renown, and with your might lead forward the host of fighting horsemen in a final war. ||

The first of these passages is recognized as posing some of the greatest difficulties of interpretation in the whole poem.[120] The connections of words, the tenses and the failure to name subject and object in the first phrase all raise questions. Radt's very fundamental,

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[118] For Byzantium's sufferings at the hands of the Thracians at a later date, see Polybius iv 44.11-46.6.

[119] Radt expressed some serious objections to this restoration and suggested that an epithet of Apollo would be more suitable here (81).

[120] See Radt's discussion, 65-75.

acute and detailed criticisms of the proposals of predecessors show that they were all in one way or another unsatisfactory. So he proposed a new hypothesis of his own, which builds on some of the insights of earlier commentators, and meets the demands of the language and of probability.

Following the brilliant suggestion of Blass, he takes the first phrase, ἀ]λλὰ . . . στρατόν, as the paraphrase of an oracle,[121] which was delivered by Hecate on the day of the new moon. Lines 105f., in which the Abderites ask for help in a final war, raise the question how they could know that the war would be final. Radt answers that it was stated in the oracle, which foretold a final victory for the Abderites.[122] Thus the oracle was recent and well known, which incidentally explains how it could be reported so obscurely, and the hymn turns to the present at this point after dealing with the glorious past.[123]

Some explanation is required of the detailed interpretation of the oracle, lines 73-5. Since σχεδόν with the dative of a noun is not attested with a verb of motion, it cannot be attached to μολόντα, so μολόντα should go with βαιοῖς σὺν ἔντεσιν, which in turn should not be simply connected with πολὺν στρατόν, since it is not normal in Greek to connect two nominal expressions in that way. Since φύρειν with a singular object cannot mean to mix or confuse, we should follow the scholiast, who states that φύρσει means 'will kill'. It is suggested that the verb acquires this meaning from a metaphorical significance, such as 'make mincemeat of'.[124] The literal rendering that results is "But near the river he (the Abderite army? a friendly deity?) will kill him (the enemy), when he has come with few arms against a numerous enemy."

Uncertain and extremely imprecise as this may be, it is possible to make some suggestions as to the actual events envisaged. It is not unreasonable to identify the river as the Nestos, the western boundary of Abderan territory. In that case, the likely enemy would be a

[121] That the exact words of the oracle are not cited, but the whole passage remains narrative, is argued convincingly by Radt, 66f.

[122] See Radt, 69f.

[123] On Radt's analysis (69), the previous episode closes with the climax of the battle at Melamphyllon, and a new topic is introduced with the new triad.

[124] For all these points, see Radt, 71.

territorial neighbour, i.e. some Thracians.[125] Radt (71) found difficulty in the consequence that this Thracian enemy is described as coming with few arms against a numerous Abderan army. The Thracians were famous for their numbers.[126] However, if we take 'few arms' as referring to the relative lack of armour and less good weaponry of the Thracians, which seems possible, that difficulty is removed. If the above interpretation is on the right lines, we see Abdera under threat of war from their neighbours, but strong enough to oppose them with a large army. It is also a sign of wealth that the Abderan army was apparently powerful in cavalry (line 104).

The date of this war is impossible to fix at all exactly. The various attempts that have been made to date Pindar's Second Paean can all be seen to be unconvincing or inconclusive.[127] Such fighting is not conceivable during the Persian rule over Thrace, so we may at least put it || after that ceased shortly after the Persian Wars.[128] It might be tentatively suggested that the power of the great Odrysian kingdom, which Hammond thinks began to be established in the 460s[129] (Abdera's relations to it are discussed above and below), might make it unlikely that this war would have occurred after *c.* 460. In which case we should have fairly close brackets, and could date the war within the 470s and 460s. But the argument is fragile, and the only really secure lower terminus seems to be the death of Pindar

[125] Koukouli-Chrysanthaki also takes the river to be the Nestos, *op. cit.* (n. 29) 88f., but her attempt to identify Abdera's enemies by archaeological survey seems too uncertain to carry conviction.

[126] *Cf.*, e.g., Hdt. v 3; Thuc. ii 95-7.

[127] See Radt's discussion, 17-19. Huxley regards *c.* 494 as a firm *terminus post quem*, since he thinks that Pindar certainty referred to the destruction of Teos after the Ionian Revolt; *Studies . . . Dow*, 152. However, that is not strictly certain; see my "Adopted Teians etc." (n. 4).

[128] Hammond believes that Persian power in inland Thrace persisted down to the 460s, when the build-up of the Odrysian kingdom began; see *Chiron* x (1980) 61. The basis for this is Plutarch's statement that Persians in the Chersonese summoned help from Thracians of the interior at a time just before the revolt of Thasos (*Cim.* 14.1). But Plutarch's information seems to conflict with Herodotus', who implies that Persian power in this area was broken by the capture of Sestus in the winter 479/8 (ix 114-18) and that only Doriscus continued to be held by Persians for any length of time after the Persian Wars (vii 106.2). Nor does the combination of Plutarch with the casualty list *IG* i^2 928 seem compelling. For the difficulties and uncertainties here, *cf.* Meiggs, *Athenian empire*, 79f., 416.

[129] See previous note. For different views on this uncertain topic, see Isaac, *op. cit.* (n. 4) 96f.

in 438.[130] So the longest possible timespan within which this war could have occurred is nearly forty years.

The second war that we know of involving Abdera and a local enemy was against the Triballi in 375. We have three sources for this event: Aeneas Tacticus 15.8-10; Diodorus xv 36.1-4; Schol. Aristides, *Panathenaicus*, 172.7, 173.17.[131] According to the first, the Triballi invaded the territory of Abdera with large, warlike forces, the Abderites sallied forth against them, fought a pitched battle and won a brilliant victory, killing many of the enemy. Then the Triballi departed, reorganized themselves and invaded again. They prepared ambushes and began to ravage the land of Abdera near the city. Because the Abderites despised them as a result of their earlier victory, they rushed out full of enthusiasm, were drawn into the ambushes and suffered terrible losses, the greatest, it was said, from a single city of that size in the shortest time.

Since Aeneas is a contemporary source expert in military matters, he must be the chief guide to the actual engagements of the war. However, Diodorus adds a number of extra features. From him we have the date, the motive for the invasion of the Triballi, famine at home, their numbers, 30,000, and their losses in the first battle and their retreat, more than 2,000. He also alone states that Abdera had Thracian allies, who changed sides in the second battle and were thus partly responsible for the defeat of the Abderites. According to Diodorus, Abdera was saved from its desperate situation by the intervention of Chabrias, who expelled its non-Greek enemies and left a strong garrison in the city. It is likely that Diodorus' source here was Ephorus,[132] another contemporary, at least, of the events described.

The scholiast to Aristides, *Panathenaicus*, also knows that Chabrias helped Abdera. This source alone provides the additional information that Maronea joined the Triballi in making war on Abdera,

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[130] There is a useful, short and well-documented account of Pindar's life in Sandys' Introduction to his Loeb edition, vii-xvi.

[131] In Dindorf's edition, iii 275, 282f.

[132] It is generally believed that Ephorus was Diodorus' source for all the Greek history in books xi-xv. This theory was very strongly established by V. A. Volquardsen, *Untersuchungen über die Quellen der griechischen und sicilischen Geschichten bei Diodor, Buch XI bis XVI* (Kiel 1868); see p. 66 for all the passages where there is a clear indication of Ephoran origin, and p. 71 for his conclusion. This view is accepted by modern scholars; see. e.g., R. Drews, "Diodorus and his sources," AJP lxxxiii (1962) 383-92, and C. I. Reid, "Diodorus and his sources." *HSCP* lxxv (1971) 205-7.

and that Chabrias reconciled the two Greek cities and made them friends and allies of Athens.[133] Most of the Aristides scholia were composed in the fourth century AD by || Sopater of Apamea,[134] and they also contain at least one fragment of Ephorus, *FGH* 70 F 211, which concerns the same general period.

At first sight it might seem prudent to trust only the one good source here, Aeneas Tacticus, in which case we should just have Abdera's war against the powerful Triballi,[135] initial success and subsequent disaster. Diodorus' authority here could be called in question, because of his extraordinary error in stating (xv 36.4) that Chabrias was treacherously killed after placing the garrison in Abdera. In fact, Chabrias died in battle at Chios eighteen years later, as Diodorus himself (xvi 7.3) and other sources relate.[136] It is also obvious that the treacherous Thracian allies of Abdera mentioned by Diodorus could be an excuse for the Greek defeat.

On the other hand, the purpose of Aeneas in mentioning the episode was to illustrate the dangers of imprudently sallying out to attack, and he could well have chosen to omit any circumstances extraneous to that topic. Most of Diodorus' additional information looks beyond obvious suspicion, and, in particular, the role of Chabrias fits well into the known historical context of the early years of the Second Athenian League.[137]

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[133] This is to combine the information in the two scholia, 172.7 and 173.17 (Dindorf). It has long been recognized that the scholion to 172.7 is misplaced; see A. Schaefer, *Demosthenes und seine Zeit* i² (Leipzig 1885) 43f. At 172.7 (= section 292 in the Loeb edition) Aristides is concerned with the events of the Corinthian War. The scholion properly belongs to 173.17 (= 297, Loeb), where Aristides lists the Battle of Naxos (376), actions round Corcyra (Timotheus, 375), the defence of Greece in Thrace (Abdera, 375), the events in Acarnania (Timotheus, 375). The present scholion to 173.17 says merely that Chabrias reconciled Abdera and Maronea, but that is easily combined with the information in the earlier one that Maronea fought against Abdera, and enables us to understand the rather confused statement about reconciliation there, which has clearly been adapted in an attempt to make the scholion fit the statement in 172.7 (= 292) that it now aspires to explain, i.e. Athens' reconciliation of Thracian kings.

[134] See C. A. Behr in the Loeb edition of Aristides, vol. i. p. xvii.

[135] Compare the damage they later inflicted on Philip of Macedon; see Justin ix 3.1-3 and Griffith in *History of Macedonia* ii 583.

[136] Reasons for such a gross error are hard to find. Eduard Meyer's suggestion that it was caused by heavy abbreviation (*Geschichte des Altertums* [Stuttgart and Berlin 1902] v 396) will hardly do, since Diodorus thinks that Chabrias' murder led to the appointment of Timotheus as admiral (xv 36.4-5).

[137] The events related by Diodorus and briefly listed by Aristides, *Panathenaicus* 297 (n. 133) seem well confirmed by the contemporary epigraphic sources, Tod ii

So a maximalist interpretation, by which all the information in our three sources (apart from Chabrias' murder) is combined, may be justified.[138] Even the minimalist strategy of confining ourselves to the facts provided by Aeneas Tacticus allows the conclusion that Abdera was still subject to the threats of large and dangerous local enemies, and strong enough to be prepared to resist them. This confirms the other indications that in the Classical period the resources of Abdera were very substantial. We may not yet have the evidence to establish the size of the city or *chora* with certainty at this time, but the large estimates given above seem to have good arguments in their favour.[139] ||

Abdera's Non-Warlike Relations with Thracians

We have seen above that it is possible that Abdera paid tribute to the Odrysian kingdom and could be regarded as part of the Odrysian king's possessions. In this connection Nymphodorus, son of Pythes, of Abdera, is a very interesting figure.[140]

123, 126. The latter is Athens' alliance with Corcyra, Acarnania and Cephallenia, made in autumn 375 and so confirming Diodorus' and Aristides' mentions of Timotheus' activities in the North West. In the former Abdera is listed as a member of the Second Athenian League at line 99, among the names at the side, which would suit its accession in 375, and that is generally assumed; see Tod's commentary, 67, and J. Cargill *The Second Athenian League* (Berkeley 1981) 42. Maronea is listed as a member in line 87, among the names at the end of the inscription, which might be thought to imply an earlier accession. However, Cargill (38) has pointed out the uncertainty of the dates of accession even of the allies in this list, so the scholiast to Aristides is not necessarily wrong in stating that Chabrias brought in both Abdera and Maronea in 375.

[138] I have not found a really full, satisfactory modern account of this war. There is no help in D. Whitehead, *Aineas the Tactician* (Oxford 1990) 139f., whose short notes follow closely I. W. Hunter, ΑΙΝΕΙΟΥ ΠΟΛΙΟΡΚΗΤΙΚΑ: *Aeneas on Siegecraft*, rev. S. A. Handford (Oxford 1927) 148, and thus miss the scholia to Aristides. Isaac mentions the war briefly without source criticism, *op. cit.* (n. 4) 106, and May gave a good short treatment, *Coinage of Abdera* 241f. C. Danov *Altthrakien* (Berlin, New York) 349, follows J. Wiesner, *Die Thraker* (Stuttgart 1963), whose account has many deficiencies. He misses Aeneas Tacticus and makes several assumptions that the sources do not justify, as that the Triballi were forced to emigrate by pressure of Celtic migration, that they went as a complete tribe with women and children (this presumably from a misunderstanding of Diodorus' πανδημεί), and that they attacked the territory of Maronea as well as Abdera. On the other hand, there is a good treatment, which respects the ancient sources, in the old work of Schaefer, *ibid.* (n. 133).

[139] See pp. 61f.

[140] He is well treated by Isaac, *op. cit.* 99-104.

Thucydides tells us (ii 29.1, 5) that, in the summer of 431, Nymphodorus, whose sister was a wife of Sitalces, and who had great influence with him, was made a *proxenos* by the Athenians, although they had previously regarded him as an enemy. The reason for their action was that they wanted to make an alliance with Sitalces. Nymphodorus went to Athens and made the alliance. At the same time the Athenians made Sitalces' son, Sadocus, who may have been, as has often been pointed out,[141] Nymphodorus' nephew, an Athenian citizen.

The following summer, 430, ambassadors from the Peloponnesians en route for the Great King went first to Sitalces, to try to persuade him to give up his alliance with Athens and campaign to save Potidaea. But Sitalces was influenced by Nymphodorus and Athenian ambassadors who were present to allow his son Sadocus to capture the Peloponnesian ambassadors at Bisanthe, before they set sail, and to hand them over to the Athenians. They were taken back to Athens and summarily killed.[142]

So we see that Nymphodorus was a Greek related by marriage to the powerful Thracian king and very influential with him. He was able to represent the Odrysian interest in negotiations at Athens, and the military help that he promised Sitalces would give was duly delivered.[143] It seems fair to compare him with Heraclides of Maronea, who in *c.* 400 was serving as the agent of the Thracian prince Seuthes (later Seuthes II), and whose actions on behalf of his Thracian master are vividly described in the seventh book of Xenophon's *Anabasis*. So Nymphodorus has been called the agent of Sitalces, and it seems right to think that that role was the reason why the Athenians regarded him as an enemy before 431.[144]

Nymphodorus' standing in his native city seems to be revealed by the coins. His name appears as a magistrate on coins of Abdera, as does that of his father Pythes.[145] The magistrate on the coins and

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[141] See Isaac, 100.

[142] We can combine Thucydides' fuller account, ii 67, with the shorter statement of Herodotus, vii 137.2-3, which nevertheless contains two facts, Nymphodorus' participation and the place of seizure, not mentioned by Thucydides. It has long been recognized that the two accounts are not in conflict, and the differences merely reflect the different interests of the two historians; see Isaac, *ibid.*, and Gomme, *Commentary* to ii 67.1.

[143] Thuc. ii 29.5; 95-101.

[144] As Isaac, 101.

[145] May, *Coinage of Abdera*, nos. 140, 141; 30, 131. *Cf.* Isaac 103.

Thucydides' Nymphodorus have regularly been identified,[146] which may not be absolutely certain. But even if that identification is wrong, we may safely conclude that other generations of the same family held high positions in Abderan public life, and it follows that Nymphodorus belonged to the elite of the city.

Nymphodorus' high status at Abdera combined with his role as agent of Sitalces establishes with certainty a close relationship between Abdera and the Odrysian kingdom. This combination would also not be out of place if Abdera was subject to that kingdom and liable to pay tribute to it. Unfortunately, the evidence is not sufficiently rich or specific to show for certain that such a situation of dependency existed, or, if it did, the degree of dependency or the details of what || it involved. We must be content, therefore, with the knowledge that the Greek colony had very close relations with the great Thracian kingdom.

Signs of peaceable Thracian influence on Abdera have been seen in religion. Although it is clear that Hecate was brought by the colonists from their Ionian homeland, the suggestion has been made that she was identified at Abdera with the Thracian goddess Bendis. There is no evidence to support that idea, however, and a major objection to it is that Bendis was normally identified with Artemis.[147]

The other deity of Abdera in whom a Thracian connection is seen is Apollo Derenos. As we have seen, the scholiast to Lycophron, *Alex.* 440, stated that the epithet was the name of a place in Abdera where the shrine of Apollo stood.[148] Danov compares the names Zeranioi, Zerania, and concludes that either the deity was local or the place had a thoroughly native name.[149] The difficulty with this view is that, though the ending -ηνός is common in local epithets in Thrace, it is also a widespread phenomenon and thoroughly

[146] As by May, 149f., if cautiously (150 n. 1), and Mattingly *Klio* lix (1977) 93, though with different dating. The difficulties for the identification perhaps caused by the chronology (Isaac, *ibid.*) are smaller with the lower dating suggested by the recently discovered 'Decadrachm Hoard'; see M. J. Price, "The coinage of the northern Aegean," *Coinage and administration in the Athenian and Persian empires*, ed. I. Carradice *BAR International Series* cccxliii (Oxford 1987) 43-7, at 45.

[147] On this topic see Radt (73) and Isaac 107f.

[148] Above p. 44.

[149] *Altthrakien* 162f., 348f., but the analogy does not seem very close. For these names see D. Detschew, *Die thrakischen Sprachreste* (*Öst. Akad. Wiss. Phil. Hist. Kl., Schriften der Balkancommission, Linguistische Abteilung* xiv, Vienna 1957) s.vv.

Greek.[150] So it is no more than speculation that Apollo Derenos is a Thracian deity assimilated to Greek Apollo, or even that the place-name is necessarily Thracian. We must conclude that clear evidence of Thracian influence on Abderan religion in Classical times does not at present exist.

It might seem paradoxical to adduce military arms and organization among peaceable relations with Thracians, but it does seem likely that Abdera's army contained peltasts. This was deduced by Mattingly from the naked warrior with a conical Thracian helmet on the reverse of May's coin no. 219, issued by the moneyer Nikostratos.[151] May's idea that the reverse plays on the moneyer's name is attractive,[152] but that in no way detracts from the force of the evidence that Abdera employed such an arm. There were many Greek cities in touch with Thrace, who either armed their own troops with the *pelta* or employed Thracians,[153] and the local influence on the military tactics and weapons used by these Greek cities is clear.

Finally, in this rather various collection of peaceable relations between Abdera and Thracians, we may note that a local Thracian ruler, Spokes, coined with his own legend but with types of Apollo and the griffin characteristic of Abdera's coinage.[154] According to May's chronology, the types on Spokes' coin would indicate period VIII of the Abderan coinage, i.e. *c.* 375/3-365/60. There are many examples of non-Greek peoples and rulers using Greek coins, coining with Greek types and striking their coins at the mints of Greek cities.[155] It is not generally possible to define the political implications, if any, of these practices, but this instance of Abdera's relations with her native neighbours was characteristic of the Greek colonial world and very common in the Thracian and Pontic regions. ||

[150] See D. Detschew, 'Die Ethnika auf -ανός, -ηνός', *Zeitschrift für vergleichende Sprachforschung* lxiii (1936) 227-40. The more recent paper of Z. Gočeva, "Epitheta des Apollon in Thrakien," *Thracia* iv (1977) 207-23, argues that many of the epithets ending in this way are probably local. Unfortunately, she misses Derenos, a bad gap.

[151] See May, *Coinage of Abdera* and Mattingly, *op. cit.* (n. 112) 98; *cf.* Isaac 103f.

[152] P. 158.

[153] See the sensible remarks of J. G. P. Best, *Thracian peltasts and their influence on Greek warfare* (Groningen 1969) 12f. Thucydides mentioned peltasts from Chalcidice, Aenus, Lemnos and Imbros, Olynthus, all places in contact with Thrace: see iv 28.4; 111.1; 123.4; 129.2; ii 79.4.

[154] See May, *Coinage of Abdera*, 243.

[155] See my remarks in "Commercial interchanges between Greeks and natives," *Ancient World* x (1984) 3-10, at 7.

Conclusion

The material discussed above shows that Abdera offers a very rich chapter in Greek colonial history. The Teian refugees succeeded in founding a colony in very difficult territory on a site named by Phoenicians and previously colonised by their near Ionian neighbour, Clazomenae. There they transplanted the political and religious institutions from Teos that we have noted above.[156] There the archaeologists have found the East Greek *prostas* house type (as opposed to the *pastas*),[157] as we should expect. The comparison with Elea is enlightening. The Phocaean refugee colony, founded at the same time and in response to the same circumstances, was also quickly successful and also provides evidence of building techniques brought from the mother city.[158] It is significant that both colonies produced major philosophers not long after their foundation. Not only did they bring advanced civilization and culture, but they soon acquired sufficient prosperity to support such activities as philosophical speculation.[159]

Also well illustrated in the case of Abdera are the two external relationships that were of fundamental importance for Greek colonies, that with the mother city and that with the neighbouring native peoples. Abdera was situated in a potentially very hostile environment,[160] and had to fight many wars against non-Greek enemies, not only in the phase of the colony's establishment, but also subsequently. However, we have also seen instances of different relationships, illustrating the *modus vivendi* which all long-lasting Greek colonies had to create with the neighbouring native peoples. If those relations included political subservience to a powerful native kingdom, as is possible, they are comparable with those of other Greek colonies on the coasts of Thrace and Scythia, such as Bisanthe, Byzantium, Mesembria in the Pontus and Olbia, all of which may at times have been subject in some degree to powerful native people.[161] There are inevitably many

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[156] P. 57.

[157] See J. W. Graham, "Houses at Abdera and Himera," *AJA* lxxvi (1972) 295-301.

[158] See my remarks in *CAH* iii^2.3, 142f.

[159] See *ibid.*

[160] Well illustrated by the failures to colonize Nine Ways before the final success in 437, when Amphipolis was established; see Thuc. iv 102.2-3; *cf.* i 100.3; Hdt. v 126; ix 75.

[161] Bisanthe: because Sadocus was able to seize the Peloponnesian ambassadors

questions about these relations of Abdera, which our evidence does not answer. Not only is the legal relationship with the Odrysian kingdom uncertain, but there are other relations with the Thracians about which we would like to be informed, such as exactly how Abdera acquired the silver which we know from the early coins to have been an important export.[162] It is not impossible that new evidence may throw light on such questions, but what we already have illustrates well the various relations that a Greek colony in this area might have with the Thracian peoples.

It is, however, the relationship with the mother city which new evidence has so notably revealed to us. We have seen that in the case of Abdera and Teos this relationship was so close as to bring the separate political existence of the two cities into question. The most striking evidence for that close relationship, the new inscription of Public Imprecations from Teos, || probably belonged to the period 479-450.[163] But we have also seen clear evidence for the same close relationship in the Teian decree about *sympoliteia* with Kyrbissos in the third century. In the period immediately after Rome's defeat of Perseus in 168 BC, when Abdera was in danger of losing some of its ancestral territory (πάτριος χώρα, lines 6, 9) to the claims of King Cotys, Teos sent ambassadors to Rome to plead Abdera's cause.[164] After their successful intervention, Abdera passed a decree honouring the ambassadors, which was to be inscribed and displayed at both Abdera and Teos. It is to the survival of the copy at Teos

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there; see above p. 66; and because the Thracian prince Seuthes was later able to promise it to Xenophon; see *Anab.* vii 2.38. *Cf.* Isaac, *op. cit.* (n. 4) 212f., who notes that Bisanthe did not pay tribute to Athens. However, it was assessed for two talents in A10 (= *IG* i^{3} 77) col. IV line 10. Byzantium: see above p. 62 and n. 118. Mesembria in the Pontus: the inscription *IGBR* i^{2} 307, recording an agreement between Mesembria and a Thracian ruler called Sadalas, shows that Mesembria paid him an annual tribute, and may have had similar relations to three of his predecessors. (The dating of this inscription is uncertain and controversial; *cf. SEG* xxx [1980] 701; 31 [1981] 678, 679; but that need not be pursued here.) Olbia: see my remarks in *CAH* iii^{2}.3, 127.

[162] See above p. 50 and n. 46. For some inevitably uncertain speculations on this topic, see Koukouli-Chrysanthaki, *op. cit.* (n. 29) 89f.

[163] See above p. 54.

[164] *Syll.*^{3} 656, but many improvements in both readings and interpretation were made by L. Robert, *BCH* lix (1935) 507-13 (= *Opera minora selecta* i 320-6) and by Herrmann, "Zum Beschluss von Abdera aus Teos, *Syll.* 656," *ZPE* vii (1971) 72-7. Thus McCabe and Plunkett were able to publish a much better text, *Teos inscriptions* 35. There is also another honorary decree of Abdera on an inscription of Teos, but it is still unpublished; see *Teos inscriptions* 38.5.

that we owe our knowledge of this event. So the relationship between the colony and mother city is not only very close, it also persisted over centuries. One event of extraordinary interest resulting from that close relationship was the refoundation of Teos by Abdera.[165]

Reinforcement and refoundation of colonies by mother cities are found in our record,[166] but this is the first express attestation of the refoundation of a mother city by a colony. The nearest we have to another recorded instance seems to be Sybaris. After its total destruction by Croton in *c.* 510, refugees went, according to Herodotus (vi 21.1), to Laus and Scidrus. Of these Laus is attested as a Sybarite colony (Strabo vi 1.1, C253) and Scidrus is with good reason thought to have been one.[167] Although we have no explicit evidence, it is also widely assumed from the coins that Posidonia, the greatest colony of Sybaris, was another refuge for those who escaped from the mother city.[168]

There were, it seems, several attempts to refound Sybaris in the fifth century. Unfortunately, the literary evidence for these is of poor quality, and it can only be supplemented or strengthened by numismatic or archaeological material at the risk of inevitable uncertainty.[169] In excavations at Sybaris a layer of alluvium has been discovered that seals the levels at the end of the sixth century, and above that the material is of the second half of the fifth.[170] This supports the

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[165] See above p. 53.

[166] See my *Colony and mother city*, especially 64-7, 144f. The refoundations in fourth-century Sicily broaden the concept from mother cities to wider kin; *cf. SEG* xii 379, 380 and R. J. A. Talbert, *Timoleon and the revival of Greek Sicily* (Cambridge 1974) 150, 204f.

[167] See T. J. Dunbabin, *The Western Greeks* (Oxford 1948) 155f.

[168] The simultaneous appearance of the Sybarite type on the reverse of the coins of Posidonia and adoption of the Achaean standard have long led to this assumption; see e.g., my *Colony and mother city*, 114. Kraay downdated these coins, putting their beginning no later than the decade 470-60; see "The coinage of Sybaris after 510 BC," *Num. Chron.* sixth series, xxxiii (1958) 13-37, at 18-20; but his arguments do not seem decisive to me, and, in any case, the assumed significance of the coins is unchanged.

If the first coins of Laus were the result of the arrival of the Sybarite refugees, as is often assumed (see e.g. P. G. Guzzo, "Tra Sibari e Thurii," *Klearchos* xviii [1976] 27-64, at 38), they would provide an analogy to support the suggested interpretation of the coins of Posidonia. But the first coins of Laus need not have been so caused; the manheaded bull is, in any case, not the same as the type of Sybaris.

[169] Kraay's bold hypotheses illustrate the point clearly. Guzzo's thorough treatment is rightly much more cautious.

[170] See Guzzo, 51.

literary record of major destruction in *c.* 510 and successful refoundation shortly after the middle of the fifth century. However, on such a vast, flat and featureless site it is always possible that evidence for habitation in the interval is buried somewhere. Nor can the various coins be assigned with confidence to the various refoundations doubtfully deduced and || dated from the inferior literary evidence.[171] So it is certainly hazardous to make historical reconstructions. However, the combination on the same coins of the types and legends of Sybaris with those of Posidonia,[172] and possibly also with those of Laus,[173] makes it a reasonable hypothesis that the two colonies were involved in attempts to refound the mother city.

It is, therefore, attractive to attribute the treaty of alliance between Sybaris and her allies and the Serdaioi, of which the only human witnesses were the city of Posidonia, to one of these occasions.[174] That would certainly explain the special role of Posidonia more easily than any circumstances we can imagine in the period before the destruction of Archaic Sybaris in *c.* 510. On the other hand, the term 'the Sybarites and their allies' reflects so perfectly a hegemonial situation[175] that it is hard to envisage its suitability to any time after that destruction. So the chronological indications of this famous document seem to be in conflict, and its date is likely to remain uncertain until some new evidence accrues.

Difficult though the evidence may be, however, the refoundation of Sybaris with the help of her colonies is an acceptable hypothesis, and to that extent we have a parallel for Abdera's refoundation of Teos.

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[171] Especially controversial is the hypothesis of an immediate re-occupation of the site of Sybaris under the rule of Croton; see Kraay, 14-16. This is well criticized by P. J. Bicknell, "The tyranny of Kleinias at Kroton," *Klearchos* xviii (1976) 5-25, at 20f., whose objections are not removed by Guzzo's concept of 'economic' re-occupation under the rule of Croton (27-31).

[172] See Kraay, 22-4.

[173] See Kraay, 17, 21-3 and, just possibly, the problematic coins discussed at 31-5, on which see also Bicknell, 23.

[174] ML 10. The argument for a late dating of this document was well put by H. B. Mattingly, "Athens and the Western Greeks, *c.* 500-413 BC" *Atti del I convegno del Centro Internazionale di Studi Numismatici* (Rome 1969) 201-21, at 209-11. The strange interpretation of the inscription by H. Van Effenterre, 'La fondation de Paestum', *PP* xxxv (1980) 161-75 (cf. *SEG* xxxi [1981] 357), may safely be ignored. It offends against both the Greek and all historical probability.

[175] See the commentary in ML.

Once we recognize the process of refoundation of a mother city by a colony or colonies, it is tempting to apply it to other occasions. For instance, two cities much more famous than Teos—Miletus and Eretria—were totally destroyed by the Persians. Both were quickly in existence again, but our evidence does not tell us how they were populated. It is certainly true that destroyed cities in the ancient Greek world seem regularly to have come to life again. Camarina in Sicily, for example, was depopulated after its war of revolt against Syracuse, according to Thucydides (vi 5.3), yet we know from archaeology that there is no break in its occupation.[176] Priene was totally depopulated by the Persians after Pactyas' revolt, yet was able, admittedly some fifty years later, to contribute twelve ships to the Ionian cause at the Battle of Lade.[177] So it might be argued that there is no need to seek explanations for the rebirth of such destroyed cities. But it is not so simple a matter as the recovery of Moscow in 1812 so vividly described in *War and Peace.* For the population of these Greek cities is stated to have been destroyed or permanently removed, not to have fled. New inhabitants must have come from somewhere.

Herodotus gives us a pretty full account of the fate of Miletus after the failure of the Ionian Revolt (vi 18-22). He says first that the Persians enslaved (ἠνδραποδίσαντο) the city. The most extreme interpretation of that would be that the adult males were killed and the women and children sold as slaves. However, more specific statements follow: 19.3, most of the men || were killed and the women and children enslaved; 20, those who were made prisoner by the Persians were taken to Susa and settled on the Red Sea at the mouth of the Tigris; the city and plain of Miletus became the possessions of the Persians themselves, while the high land was given to the Carian Pedaseis; 22.1, so Miletus was emptied of Milesians; 22.3, those Milesians who had fled joined Samians in a colonial venture in Sicily.

We see from these statements that while most Milesian men were killed, some succeeded in escaping and others were taken alive by the Persians and transplanted. The Milesians who were among the Persian forces at Mycale (Hdt. ix 99.3; 104) seem most likely to have come from this last group. They knew the local terrain but were distrusted by the Persians. Herodotus' statements do not seem to favour the view of some modern scholars that Miletus existed between

[176] Dunbabin, *Western Greeks* 106f.

[177] Hdt. i 161; vi 8.1. We are not told how it was resettled.

493 and 479 and was inhabited by pro-Persian Milesians.[178] We do not need such a hypothesis to account for the Milesians at Mycale, who were not regarded as pro-Persian. The main basis for the theory is the supposed starting date for the great list of *stephanephoroi*, the eponymous magistrates at Miletus.[179] If we calculate back from the known dates of Alexander's and Asander's tenures in 334/3 and 314/13, and from Antigonus' freeing of the city in 313/12, and allow one year to each name, we reach 525/4.[180] This date is thought to fit well the time of the Parian regulation of the city after two generations of civil strife (Hdt. v 28-9), these two generations being assumed to follow the tyranny of Thrasybulus. Thus we are invited to conclude that the list, which reveals no break, shows that the constitution continued to function without any hiatus from 525/4 onwards. So there was no break in the political life of the city after its destruction at the end of the Ionian Revolt.

This ingenious argument is not compelling. That there is no hiatus in the list does not prove that the recorded names reflect an unbroken succession of years. As Rehm rightly remarked, we cannot tell if there was a gap unstated by the list, and he added that there is a curious absence of the names of the great men of the Ionian Revolt period. So he thought the problem insoluble.[181] Nor is it necessary that the list of eponymous magistrates began at the time of the Parian regulation of the city, nor that Herodotus' two generations began with the end of the tyranny of Thrasybulus, the

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[178] See, for instance, H. Berve, *Die Tyrannis bei den Griechen* i (Munich 1967) 106, who thinks that the city gradually grew up again in the 480s and tentatively suggests that the tyrant Aristogenes, stated by Plutarch (*de Herod. mal.* 21, 859 d) to have been expelled by the Spartans, might have been a pro-Persian creature, who ruled at that time and was expelled after Mycale. But the Plutarch passage is full of uncertainties and unknowns and there are many earlier possible occasions.

[179] See F. Hiller von Gaertringen, *RE* s.v. Miletos (1933) 1595; B. Virgilio, *Commento storico al quinto libro delle 'storie' di Erodoto* (Pisa 1975) 63-4; N. Robertson, "Government and society at Miletus, 525-422 BC," *Phoenix* xli (1987) 356-98, at 375-7. This argument is widely accepted; see, e.g. P. Tozzi, *La rivolta ionica* (Pisa 1978) 205; J. M. Balcer, "Miletos (*IG* i^2 22 [*IG* i^3 21]) and the structure of alliances," *Studien zum attischen Seebund*, ed. J. M. Balcer (Konstanz 1984) 11-30, at 16-18.

[180] See A. Rehm and G. Kawerau, *Das Delphinion in Milet* (*Milet* i.3, Berlin 1914) 241f.

[181] *Ibid.* Balcer's arguments (*op. cit.* 17) that members of three or four aristocratic families served either before and after or during and after the Revolt, and that two brothers of Aristagoras held the office, depend on the assumption that there was no gap unstated by the list.

dates of which are not precisely known.[182] These theoretical objections are, however, less strong than Herodotus' plain statements. The inhabitants of Miletus were all removed in one way or another in 494/3 and the Persians themselves possessed the city.

The destruction by the Persians in 494 has been recognized at various places excavated in the city.[183] After that destruction the area of the temple of Athena was in ruins for a sufficient time for a sand-dune to develop. The new temple that was built to replace its destroyed || predecessor has been dated by architectural style to the second and third quarters of the fifth century. So the period when the sanctuary was in ruins has been taken to be 494-479.[184] If this archaeological argument is right, and can be extrapolated over the city as a whole, it supports the clear statement of Herodotus that Miletus was emptied of Milesians (vi 22.1). We must agree with those who think that the city was not refounded till 479.[185]

Founders of that reborn city will certainly have included those Milesians who escaped from the Persians at Mycale, and may also have included some who decided to return from Sicily. But, if we follow Herodotus, these would be too few to refound the city. We know that Miletus had good and close relations with her colonies, for some of which *isopoliteia* with the mother city is attested in later documents.[186] Such new inhabitants would have the advantage that they shared the same ancestry, language, cults and customs with the remnants of the citizens of Miletus. They would also, incidentally, be indistinguishable in our onomastic records.

As to Eretria, Herodotus relates that after the city was captured in 490 the people were enslaved (vi 101), and debris of the Persian destruction is founded by excavators all over the the site of the ancient city.[187] Herodotus is not to be corrected by accepting the

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[182] On the dates of Thrasybulus see Virgilio, *ibid.*, and Robertson, 376. J. M. Cook, *CAH* iii².3, 201, rightly states that we do not know the dates of the civil strife or of the Parian arbitration.

[183] See Tozzi, *op. cit.* 77f. and plate xiv.

[184] See A. Mallwitz and W. Schiering, "Der alte Athena-Tempel von Milet," *Ist. Mitt.* xviii (1968) 85-160, at 122-4.

[185] See, e.g., recently, H. W. Parke, "The massacre of the Branchidae," *JHS* cv (1985) 59-68, at 61, and R. M. Cook, "The Francis-Vickers chronology," *JHS* cix (1989) 164-70, at 168.

[186] I discuss these relations in *Colony and mother city*, chapter 6. See now Ehrhardt, *Milet und seine Kolonien*, 233-40.

[187] See P. Auberson and K. Schefold, *Führer durch Eretria* (Berne 1972) 30.

apparently more specific evidence that the captives numbered only 780, young and old, men, women and children, and that the majority of the population escaped to the mountains of inland Euboea, which is provided by Philostratus in his *Life of Apollonius of Tyana* (i 24).[188] The passage is as patently fictitious as the rest of the work. Herodotus follows the fate of the captives until their arrival at Susa and settlement by Darius at Arderikka (vi 107.2, 115, 119), noting that they still lived there in his own day and spoke their native language. Only ten years after its own destruction in 490 Eretria had sufficient citizens to man seven ships against the Persians in 480, and to provide a small contingent in the following year to the Greek forces at Plataea.[189] Even though Philostratus should be ignored, it is possible that some of these men had succeeded in escaping in 490. Once again, however, Herodotus' information seems to preclude the possibility that any remnant of the citizen body remaining free was sufficiently numerous to refound the city. Of Eretria's relations with her colonies we know very little, but, if the colonial relationships of her near Euboean neighbour, Chalcis, are any guide, they would have been close and good.[190] Another factor to remember is the proximity of Eretria's colonies.

Miletus and Eretria were Ionian cities, like Teos and Abdera. The refoundation of Teos by Abdera may, therefore, be put forward as a possible analogy to allow the hypothesis that the rapid rebirth of Miletus and Eretria also owed something to an influx of new citizens from colonies with whom close relations were maintained.

The very close relationship between Abdera and Teos attested by the new inscription of II Public Imprecations from Teos has been pronounced exceptional,[191] and, although we were able to find an analogy—Paros and Thasos—which may in a general way be comparable, it is certainly true that there is no case in our record precisely similar to the relationship revealed by the new inscription. There is the

[188] Auberson and Schefold, *ibid.*, do follow Philostratus.

[189] Hdt. viii 1.2, 6.2; ix 28.5 (600 Eretrians and Styrians).

[190] On the relations of Chalcis and her colonies, see my *Colony and mother city*, 18, 76. The remarkable solidarity of Chalcidian colonists, on which I commented in *CAH* iii^2.3, 193, is hard to explain without close relations with the mother city.

[191] See Herrmann, 27. Ehrhardt, *op. cit.* 234f., takes the same view, but his argument for so doing, that no contacts are provable between Teos and Phanagoria, is obviously *e silentio*. J. and L. Robert, *Journ. des Savants* (1976) 212f., while seeing the banning clause in the decree about Kyrbissos as a particular example of a very close colony-metropolis relationship, think that it was "perhaps very rare."

further question, however, whether the case would still be exceptional if we possessed a greater proportion of the full historical experience of Ancient Greece than our present minute fraction. When I discussed this general issue on an earlier occasion,[192] I stated that the relatively small quantity of evidence that we possess for the importance and effectiveness of the colony-metropolis relationship was shown not to be exceptional by the new evidence from inscriptions. For, if new inscriptions reveal new instances where the relationship was close and effective, it is fair to conclude that, if we had more evidence, we would have more examples. The new inscription from Teos seems to me very emphatically to confirm that argument.[193]

So I doubt if the relationship attested by that inscription was necessarily exceptional in the world of Ancient Greece. It seems better to add it to our other evidence which points to very good and close relations between Ionian colonies and mother cities, and conclude that, when that tie existed, those cities could adopt, without any element of hegemony, institutions that bridged the exclusiveness of separate political entities.

Finally, it is worth noting that, rich as the evidence about Abdera already is for the historian interested in Greek colonies, it may be hoped that yet more will emerge in the future. The large open site will continue to be investigated archaeologically and still has much to reveal.[194] ||

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[192] *Colony and mother city*, 212f.

[193] *Cf.* also my remarks in *Gnomon* lix (1987) 128f.

[194] I am most grateful to my friends, Professors T. J. Figueira and N. G. L. Hammond, who kindly read a draft of this paper and made many helpful suggestions.

15. A DEDICATION FROM THE CHERSONESE AT OLYMPIA

κεῖνται δὲ καὶ ἄλλα ἐνταῦθα ἄξια ἐπιμνησθῆναι μάχαιρα ἡ Πέλοπος χρυσοῦ τὴν λαβὴν πεποιημένη καὶ εἰργασμένον ἐλέφαντος κέρας τὸ Ἀμαλθείας ἀνάθημα Μιλτιάδου τοῦ Κίμωνος, ὅς ⟨τὴν⟩ ἀρχὴν ἔσχεν ἐν χερρονήσῳ τῇ Θρᾳκίᾳ πρῶτος τῆς οἰκίας ταύτης· καὶ ἐπίγραμμα ἐπὶ τῷ κέρατί ἐστιν ἀρχαίοις Ἀττικοῖς γράμμασι·

Ζηνί μ' ἄγαλμ' ἀνέθηκαν Ὀλυμπίῳ ἐκ χερονήσου
τεῖχος ἑλόντες Ἀράτου· ἐπῆρχε δὲ Μιλτιάδης σφίν.

Paus. 6.19.6

As has often been observed,[1] Pausanias' account of the ivory horn of plenty[2] that was preserved in the Sicyonian Treasury at Olympia contains an obvious error. Herodotus' well-known story of the colonization of the Thracian Chersonese (6.34-38) makes it clear that the Elder Miltiades, the son of Cypselus, established the family's rule there, not his nephew, Miltiades the Younger, son of Cimon and victor of Marathon. The same confusion is found elsewhere in late sources,[3] and it seems probable that Pausanias had no more information than the dedicatory inscription that he quotes.[4] ||

It is difficult to decide whether Pausanias' mistake lay in the patronymic or in the statement that the dedicator established the family's rule in the Chersonese. In favor of the view that Miltiades, son of Cimon, made the dedication may be the Corinthian helmet found at Olympia and inscribed Μιλτιάδες ἀ̣νε̣[θ]ε̣κεν[:τ]ôι Δί.[5]

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[1] See, e.g., Hitzig and Blümner 1904, 2.2:630-31.

[2] For the horn of Amalthea see the thorough article by Wernicke in *RE* 1.2:1720-23. An early account is in Pherecydes (*FGrH* 3 F 42).

[3] E.g., Nep.*Milt.* 1-2; Σ Aristid. Ὑπὲρ τῶν τεττάρων 168.3 Dindorf (3:551-52), where the scholiast must be thinking of Miltiades, son of Cimon, since he is the subject of the passage in Aristides on which he comments.

[4] Kinzl 1968, 113-14. In detailed studies of some of the inscriptions copied at Olympia by Pausanias, Gallavotti (1978a, 1979) suggests that he may have been using a collection that already existed. His main arguments in favor of this hypothesis (1978a, 3) are the facts that there are hardly any inscriptions quoted in the books about Athens and that when Pausanias does refer to an inscription at Olympia that he clearly knows personally, he merely paraphrases in prose; e.g., the inscription on the base of the Nike of Paeonius; see 5.26.1. Gallavotti does not treat the dedication at 6.19.6.

[5] See Kunze 1956, 69-74; for the inscription, Lazzarini 1976, no. 114. The

While a helmet of this style could have been made in the lifetime of Miltiades the Elder, as Kunze stated, both shape and inscription better suit the time of the Younger Miltiades.[6] The two unusual features of the dedicatory inscription, the omissions of both the *ethnikon* and the name of the enemy, if they need explanation, surely find it best in the assumption that the helmet formed part of the booty of Marathon, a battle so famous that it needed no further indication than the name of the victorious Athenian commander.[7] One recalls the self-confident use of name alone in the notorious dedicatory inscription of Pausanias the Regent after Plataea (Thuc. 1.132.2). Whether or not that is right, if the helmet was dedicated by the Younger Miltiades, as seems most probable, the analogous use of the name Miltiades alone might suggest that he also dedicated the horn of Amalthea.

Such a conclusion would, however, go against the consensus of published opinion on the subject, which prefers Miltiades the Elder.[8] It is true that most of those commentators wrote before the discovery of the helmet just mentioned. Even so, the arguments in favor of the preferred identification || were hardly decisive. It was felt, first, that Pausanias would more easily make a slip in the patronymic than perpetrate a whole false statement. That may well be right in terms of probability but need not have been so in fact. The second argument seems more substantial. Pausanias states at 6.10.8, where he

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restoration of the interpunctuation is justified by Kunze by the spacing of the letters but is perhaps rather bold.

[6] See Kunze 1956, 70-71. The helmet is very similar to nos. 151-52 in Kukahn 1936, pl. 4.1-2. Those were found in a Corinthian grave, with Black Figure pottery dated to the beginning of the fifth century (Kukahn 1936, 46). Kukann established that this form of helmet came in about the middle of the sixth century (45-47). Precise dating is also made difficult by the long working life of a Greek helmet; see Pflug 1988, 66-67.

[7] Kunze's argument that Miltiades would not have dared to omit the *ethnikon* after his return to Athens, since he would again arouse the fear of tyranny (cf. Hdt. 6.104.2), seems unconvincing. After Marathon, Miltiades' prestige made him untouchable until his failure at Paros (Hdt. 6.132-36). And Kunze's explanation for the two omissions—that the helmet was not part of dedicated booty but Miltiades' own, which he had worn in battle—is not only without support, but seems improbable and farfetched. (It is repeated by Jucker 1964.) In any case, Kunze himself later discovered another helmet of the same period, which bears an inscription of precisely the same form (Δείνανδρος [τ] ὀι̣ Δὶ 'Ολυνπ[ί]οι) and only differs from Miltiades' in that the dedicator is unknown to us; see Kunze 1967, 107-8; *SEG* 24:309. This might suggest that we do not need to look for special explanations for the form of Miltiades' dedicatory inscription.

[8] See, e.g., (to keep to modern times) *RE* 2.1:400; Kirchner 1901, no. 10209;

is talking about winners of the chariot race, that he will give Miltiades' dedications in another part of his work. Since the passage 6.19.6 is the only place in our extant text where a dedication of a Miltiades is mentioned, some have concluded that it must represent the fulfillment of the undertaking of 6.10.8.[9] In that case the dedicator of the horn of Amalthea should be Miltiades the Elder, a winner of the chariot race (Hdt. 6.36.1). However others have justifiably objected that the fit between the two passages is not good.[10] At 6.10.8, Pausanias speaks of dedications in the plural and clearly implies that they related to victories in the chariot race. So it seems rather unlikely that the dedication of 6.19.6 was in his mind and better to assume that the promise of 6.10.8 was nor kept. The weakness of these two arguments does not, of course, exclude the possibility that the dedicator of the horn of Amalthea was Miltiades the Elder. We know that he fought various campaigns in the Hellespontine region,[11] any one of which might have included the capture of Ἀράτου τεῖχος.

It might be thought that those words, Ἀράτου τεῖχος, should offer another approach to the problem.[12] The name Aratus is certainly Greek, but we know of no individual of that name who might be involved in the right area at the right time.[13] In any case, such

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Hitzig and Blümner 1904, 2.2:630; *RE* 15.2:1680-81; Berve 1937, 39; von Blumenthal 1937 (by implication); Bengtson 1939, 13 n. 4; Kahrstedt 1954, 8; Kunze 1955, 7-8; Kunze 1956, 70 n. 4.

[9] Kunze 1955, 7-8; Kunze 1956, 70 n. 4; but the argument is an old one; cf. Hitzig and Blümner 1904, 2.2:584-85 (n. to 6.10.8).

[10] Hitzig and Blümner 1904, 2.2:584-85 (n. to 6.10.8); Kinzl 1968, 113-14 n. 6.

[11] Hdt. 6.37.1: ἀποτειχίσας ὢν τὸν αὐχένα τῆς Χερσονήσου ὁ Μιλτιάδης καὶ τοὺς Ἀψινθίους τρόπῳ τοιούτῳ ὠσάμενος τῶν λοιπῶν πρώτοισι ἐπολέμησε Λαμψακηνοῖσι. Stein may have been overprecise in glossing τῶν λοίπων "sc. πολεμίων," but the indication of various military involvements is clear.

[12] Von Blumenthal (1937, 476-77) conjectured Ἀράζου, in place of Ἀράτου, arguing from Ἄραζος in St. Byz., a city πρὸς τῷ Πόντῳ, named after Ἀράζου τοῦ ἡγησαμένου, and from the easy confusion of zeta and tau in old Attic lettering. Much earlier Ἀράπλου had been suggested (see Preger 1891, 54 n.) on the basis of ps.-Scylax 67, Ἄραπλος, but that reading is itself disputed; see *GGM* 1:n. *ad loc.* Editors have rightly declined to admit these conjectures into their texts, though Ἀράζου was accepted by Bengtson (1939, 13 n. 4), and Kahrstedt admitted it as an alternative possibility (his Ἀράξου being presumably a misprint), though without acknowledgment (1954, 10). Kinzl (1968, 113 n. 6) preferred Ἀράπλου.

[13] Cf. Hitzig and Blümner (1904, 2.2:630), who even maintain that our ignorance of any individual so named in this period justifies the assumption that it certainly was just a place name, *Aratouteikhos*. But that grossly overestimates the extent of our knowledge. Better the simple confession of ignorance, as, e.g., by Berve (1937, 39) and Kahrstedt (1954, 10).

strongholds with such names were typical of the region,[14] so the name in itself tells us nothing definite || about the enemy who held it. Either the Elder or the Younger Miltiades could well have taken such a place from either Greek or barbarian opponents, and it is easy to excogitate more complicated possibilities.

The evidence and arguments so far considered seem insufficient to decide which Miltiades made the dedication reported by Pausanias, and those scholars who have left the question open seem well advised.[15] However, this is not the only historical question raised by the dedication. The wording of the inscription has been brought into the argument over whether there existed a "state" of the Chersonesitans in the sixth century.[16] Kahrstedt argued that the description of the dedicators as "the men from the Chersonese" was chosen not in order to avoid the term *Chersonesitans* as incorrect but for metrical reasons.[17] The reading and translation of the passage are involved in this argument.

Although he gave no references, Kahrstedt accepted the conjecture, οὑκ (= οἱ ἐκ) for the ἐκ of the manuscripts. This reading was proposed long ago by Philipp Buttmann and was designed, like Preger's alternative, οἱ 'κ, to provide acceptable Greek for the translation "the men from the Chersonese dedicated me as an offering to Olympian Zeus, having taken the fort of Aratus."[18] According to Threatte, the crasis οὑκ is not attested in Attic inscriptions as representing οἱ ἐκ, and οἱ 'κ is similarly unexampled.[19] In the most recent

[14] Cf. ps.-Scylax 67, where there are several examples, and Hdt. 9.115, where Sestos is called a τεῖχος.

[15] E.g., Rumpf 1936, 63; Friedländer and Hoffleit 1948, no. 52 nn.; Kinzl 1968, 113-114; Isaac 1986, 172-73.

[16] See Kahrstedt 1954, esp. 8-9 (with references to earlier literature); Ehrenberg 1946 121-28.

[17] Kahrstedt 1954, 8.

[18] Butmann's conjecture appears first in the apparatus of Bekker's edition of 1826-27 and was apparently not published separately. Presumably it was one of the ingenious conjectures of his friends, which Bekker admitted into his edition; see the remarks of Schubart and Walz 1838, xi. It is curious that von Blumenthal (1937, 476) proposed the same conjectural emendation without any reference to Buttmann. He argued that in old Attic οὑκ would be written hoκ, which would easily be changed into ἐκ. Preger (1891, no. 54; note "*scripsi*") defended his introduction here of prodelision (or aphaeresis) on the grounds that Attic writers did not observe the rule about crasis so strictly that they did not depart from it to avoid ambiguity. But he offers no instances of what was in his mind.

[19] Threatte 1980, 426-27, 429-34.

critical edition of Pausanias, both conjectures are silently condemned by their absence from the apparatus.[20]

The absence of the article has not deterred translators from rendering the passage as if it were there, as a few examples show:

> Me Iovi Olympiaco posuit donum Cheronesus,
> Miltiadis ductu, quum moenia cepit Arati. ||
>
> I was dedicated as an offering to Olympian Zeus by the men of Chersonese
> After they had taken the stronghold of Aratus: Their leader was Miltiades.
>
> To Olympian Zeus was I dedicated by the men of Chersonesus,
> After they had taken the fortress of Aratus. Their leader was Miltiades.
>
> They from the Chersonese set me up as a delight to Olympian Zeus
> after seizing the fortress of Aratus; Miltiades was their commander.
>
> Consecrated to the delight of Olympian Zeus by Chersonesians
> who took the fortress of Aratos; Miltiades commanded.[21]

However, this rendering does not seem likely to be correct. If we keep to the transmitted text and observe the natural order of the Greek, it is the ἄγαλμα that is from the Chersonese. To give the place of origin of a dedication with ἐκ/ἐξ is a common feature of Archaic wording. Take, for instance, the inscription on the gold bowl of the Cypselids: Ϙυψελίδαι ἀνέθεν ἐξ Ἡρακλείας.[22] So a close translation of our epigram should read: "To Olympian Zeus they dedicated me, a pleasing gift from the Chersonese, those who took Aratus' fort. Miltiades was their commander.[23] ||

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[20] Rocha-Pereira 1977.

[21] Respectively, Schubart and Walz 1838 (although the decision to translate into Latin hexameters may be constraining, *Cheronesus*, as the subject of the verb of dedicating, must be understood as a community, state, or the like); Frazer 1898, 312; W. H. S. Jones 1933; Friedländer and Hoffleit 1948, no. 52; Levi 1971, 2:339.

[22] *SEG* 1:94.

[23] In all the works I have consulted, the epigram appears to have been understood in this way only by Krafft (1877, 134), who paraphrases: "die Gabe aus dem Chersones gesandt worden, nachdem die Eroberung eines Ἀράτου τεῖχος, dessen Lage nicht bezeichnet werden kann, unter Anführung des Militades gelungen war." It is unfortunately not strictly possible to be sure how he took the Greek in detail from such a loose paraphrase, but it is obviously possible that he understood ἐκ Χερονήσου with ἄγαλμα. Isaac's very literal translation (Isaac 1986, 172-73: "They dedicated me as a delight to Olympian Zeus from Chersonesos, having taken *teichos* Araton [sic]. Their leader was Miltiades") might suggest that he also understood the epigram in this way. But his discussion shows that to be an illusion, for he uses the epigram to prove that Miltiades (I or II) was a tyrant over the people

The absence of the definite article with ἑλόντες would certainly seem strange in classical prose,[24] but it is not so in poetry, where the example of Homer may be presumed to be one reason for its frequent nonexpression.[25] Another reason suggested for the common omission of the article in verse is that it is a feature of high style to avoid simple expressions that only increase understanding.[26] That explanation would be particularly apt in the case of our epigram, which belongs to a type that is mildly enigmatic and holds back information about the dedicator(s).

A striking example of this type is the Attic dedication on the base of a bronze statuette, which has been dated by its letter forms to the end of the sixth century:

πᾶσιν ἴσ' ἀνθρόποις hυποκρίνομαι hόστις ἐ|ρο|τᾶι·
hός μ' ἀνέθεκ' ἀνδρον· 'Αντιφάνες δεκάτεν.[27]

Hansen has rightly followed the interpretation of Crönert,[28] which leads to the following translation: "To all men I answer alike, whoever asks, who among men dedicated me? Antiphanes, as a tithe." The epigram envisages a dialogue with its reader, who has to proceed to the end of the rather riddling, teasing words before he receives the answer he seeks. On the bronze, this character of the epigram is made much more obvious by the disposition of the inscription, since the reader has to follow the writing almost twice round

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of the Chersonese: "The one original document we have, signed by a Miltiades . . . shows us an absolute ruler over an unspecified group of people on the Chersonese." So he follows the traditional interpretation.

[24] Cf. KG 2.1:608-9, where the rule is stated that the article is used with participles to express a determined person or persons, and that when omitted it shows that they are indeterminate. This is often clear from the examples cited, but contrast Pl. *Lg.* 7.795b (διαφέρει δὲ πάμπολυ μαθὼν μὴ μαθόντος καὶ ὁ γυμνασάμενος τοῦ μὴ γυμνασαμένου), which is naturally found 'auffallend'.

[25] See, e.g., Friedländer and Hoffleit (1948, 52), who reject the emendation οὐκ on the grounds that the article is not necessary in the epic style; Bers (1984, 11), who writes of the "omission (or, better, nonexpression) of the definite article in poetry as compared with all varieties of prose"; and Wackernagel (1920, 2:147-50). For the details of Homeric usage in this respect see Chantraine 1963, 2 sec. 466: cf. Wackernagel 1920, 2:128-29.

[26] Wackernagel 1920, 2:148.

[27] Hansen 1983, no. 286: Lazzarini 1976, no. 658 (though with markedly inferior readings to Hansen's). Lazzarini follows the suggestions of Beloch reported in *SEG* 3:45. The same errors vitiate the text of Friedländer and Hoffleit (1948, no. 131) and destroy the value of their translation.

[28] W. Crönert 1925, 2050.

the base before reaching the end of the maze.[29] This operation may even have been made slightly more difficult by the presence of the now missing statuette. Lazzarini is able to adduce another good example of this type of epigram, her no. 688 from Halicarnassus, which is formally similar || and of the same period.[30] The same character can be seen in our epigram. Who dedicated it is not expressed at all with the verb. Partial information comes with the participial clause in the second line. But the full revelation is given only in the final phrase—and even then only for readers with the knowledge and intelligence to understand—by the naming of Miltiades. The dedicators were his followers. One would love to know how the words were disposed on the object.

If this interpretation of the dedicatory epigram is right, does it help to clarify the inscription's historical significance? The dedicators are no longer defined as "the men of the Chersonese," so the document does not seem to contribute to the debate about the existence of a state of the Chersonesitans. However, Herodotus makes it plain that there was such a political entity under the rule of the Philaids,[31] and his testimony is clearly confirmed by coins,[32] so we do not need the epigram in that debate. It may be significant, nevertheless, that the dedicators are only defined as men commanded by Miltiades, without any *ethnikon* or other designation.[33] We know that Miltiades the Elder, the *oikistes*, led both Thracian Dolonci and Athenians on his initial expedition (Hdt. 6.36.1). We also know that the Younger Miltiades employed a force of mercenaries (Hdt. 6.39.2). It is probable that both leaders, in all their military actions, used

[29] See the drawing in *IG* I[2] vi, and photograph; Lazzarini 1976, pl. 2.3.

[30] Lazzarini 1976, 129-30.

[31] 6.38.1 (the Chersonesitae establish the worship of Miltiades the *oikistes*); 39.2 (the Chersonesitae . . . from all the cities, the men in power, come together to join in mourning with the newly arrived Miltiades the Younger); 103.4 (Miltiades the Elder described as *oikisties* of the Chersonese); 4.137.1 (Miltiades the Younger described as the *strategos* and tyrant of the Cheronesitai on the Hellespont). It may also be significant that he calls the inhabitants of Sestos Chersonesitae (9.118.2, 9.120.1). There are, naturally, many questions of detailed interpretation, which it is not necessary to go into here. See the references in n. 16 above.

[32] Kahrstedt 1954, 8-9. For these Attic weight tetradrachms see Head 1911, 257; Seltman 1955, 86-87; Kraay 1976, 158. The inscription XEP on one of these rare coins establishes their origin clearly enough. Athena's head on the obverse has an obvious reference; the lion with head turned back on the reverse is thought to refer to the Milesian origin of Cardia. The general opinion of the numismatists is that the issue belongs to the reign of the Younger Miltiades.

[33] Berve 1937, 39.

forces of varied origin and status, for whom the sole unifying factor was the person of their commander. This may be the reality that the words of the dedication accurately reflect.

At some risk of pressing the evidence too hard, one could also employ the following argument. If the object dedicated was "from the Chersonese." and if it formed part of the booty taken at the capture of the fort of Aratus,[34] || that fort should be situated within the Chersonese.[35] Even that conclusion would still leave too many possibilities for the campaign to be pinpointed with any certainty. The Younger Miltiades had to establish his rule by trickery and force (or the threat of force [Hdt. 6.39.2]), and it is possible that his later flight from the Scythians and subsequent return involved fighting within the Chersonese, even though there is no record of it in Herodotus' brief account (6.40). But if there was one time when it is highly likely that it was necessary to take by force fortresses within the Chersonese, it was when Miltiades the Elder initially established his rule over the peninsula, and made it secure from the threats of the Apsinthians. So at the least one might say that the dedication could have been part of the booty taken on that occasion by the forces of Miltiades the Elder.

But these are only possibilities, and it would be unwise to press such exiguous and uncertain evidence too hard. This reexamination of the historical significance of the dedication reported by Pausanias could hardly be expected to yield new and certain historical conclusions. It will be enough if a more correct understanding of the epigram excludes false historical deductions and defines more accurately the possible historical circumstances of an important, fascinating, and tantalizing dedication. ||

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[34] Dr. E. Knauer has suggested to me that the horn of Amalthea was an actual drinking horn, part of the booty captured at the fort, rather than a work of art created specially for the dedication and paid for from part of the proceeds of the spoils. It does not seem to me possible to choose between these possibilities. Rumpf (1936, 62-63) showed that artistic representations of horns of plenty certainly existed by the time of this dedication. However, it is also true that drinking horns were prevalent in the area and at the time; see Ath. 11.476; Xen. *An.* 7.3.21-25; Jacobsthal 1944, 1:111-14. And some of the ancient accounts make the horn of Amalthea an actual bull's horn; Pherecydes (*FGrH* 3 F 42); Str. 10.2.19 (C 458).

[35] Contrast Berve (1937, 39). who, using the received translation, suggested that ἐκ Χερονήσου might imply that Aratus' fort was outside the Chersonese proper, presumably thinking that, because the men were from the Chersonese, the fort should be outside it and so possibly a stronghold of the Apsinthians. But he rightly

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recognized the uncertainty of such a supposition, which is excluded by the translation I am advocating.

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16. THE FIFTH-CENTURY CLERUCHY ON LEMNOS

It is well known that our reconstruction of Athenian colonization in the fifth century is hindered by great difficulty in establishing the status of many of the settlements.[1] Among these problems one of the most disputed concerns the fifth-century Athenian settlement on Lemnos.[2] It is not my intention here to review again all the evidence, but simply to draw attention to, and try to determine the significance of, two inscriptions[3] which must be added to the material bearing on the problem. These inscriptions were thought by the original editor, Segre, to prove that there was a cleruchy on Lemnos well before the date in the middle of the fifth century which has been regularly proposed,[4] and he is followed by Robert,[5] yet two recent discussions,[6] both of which deny that there was a cleruchy on Lemnos at all in the fifth century, take no account of the new evidence.

The two inscriptions record legal encumbrances, and both use the normal Athenian formulae of the πρᾶσις ἐπὶ λύσει.[7] The first is very fragmentary and only known from a single copy. Segre argued from the letter forms that it should be dated to before 480.[8] The second is much better preserved and includes the words Εὐαινέτωι Ἐρχιεῖ, that is name and Athenian demotic. Because the letters were very roughly cut it cannot be precisely dated by its script, but Segre's argument from its place of discovery, by a tomb of the end of the

[1] H. Schaefer's plea (*Heidelberger Jahrbücher* 1960, IV, 78 n. 4) for a new treatment of the subject which would distinguish sharply between apoikia, epoikia and cleruchy seems not to recognise that such distinction may be impossible; cf. Ehrenberg, *CP* XLVII 1952, 143ff.

[2] The most useful discussions in the modern literature seem to me those of Ehrenberg, *Aspects of the Ancient World*, Oxford 1946, 128ff., and *ATL* III 289ff.

[3] Published by M. Segre, *Annuario della R. Scuola di Atene* XV-XVI 1932/3 (appeared 1942), no. 11, pp. 305f., and no. 12, pp. 306f.

[4] Pp. 305f.

[5] *REG* LXII 1949, 133.

[6] A. H. M. Jones, *Athenian Democracy*, Oxford 1957, 172f., and F. Gschnitzer, *Abhängige Orte im griech. Altertum*, Munich 1958, 102 n. 3.

[7] See M. I. Finley, *Land and Credit in Ancient Athens*, New Brunswick 1951, 31ff., 122ff.

[8] *Op. cit.* 305f.

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fifth century, seems convincing. It was probably already out of use before the tomb was built; and it should therefore be dated to some time before 400. As the sigmas on the stone have four bars, we may add that it was probably not inscribed before *ca.* 445.[9]

It is necessary first of all to distinguish between the two inscriptions. Finley, for instance, wrongly combines them and says that the Athenian demotics[10] show that there was a cleruchy on Lemnos before 480.[11] The argument from the demotics only applies to the later inscription. ||

As for the earlier inscription, Finley is no doubt right to note that letter forms, when the stone itself has been lost, must be considered uncertain evidence of date,[12] but even if the dating is accepted it seems doubtful if the documents proves that the Athenian settlement of that time was a cleruchy. It shows that the Athenian colonists maintained Athenian legal and commercial practices in their new home, but this is not in the least surprising and tells us nothing precise about the status of the Athenian settlement on Lemnos at this date.

The second inscription seems to be much more significant, simply because at least one of the participants in the transaction had an Athenian demotic. It is true that citizen divisions were one of the arrangements of the mother city normally reproduced in a Greek colony, and also that membership of a deme at Athens after Cleisthenes' reform was determined on a hereditary, not a local, principle. On the other hand a deme was an existing part of Attica, and Athenian citizenship was based on membership of a deme, so that it may be said that a man's character as an Athenian citizen was expressed in his Athenian demotic.[13] In view of this, it seems impossible to deny that Euainetos of the deme Erchia was an Athenian citizen, and his financial dealings have left reasonably clear evidence of a cleruchy on Lemnos in the second half of the fifth century.

It seems right to conclude, therefore, that though the new evidence does not determine the status of the first Athenian settlement on Lemnos, it makes untenable the thesis that there was no cleruchy on Lemnos at any time in the fifth century. ||

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[9] See Klaffenbach in Kirchner, *Imagines Inscriptionum Atticarum*[2], Berlin 1948, notes to no. 30.

[10] A second demotic is conjecturally restored by Segre p. 307.

[11] See *op. cit.* 200 n. 26.

[12] *Loc. cit.*

[13] See Arist.*Ath.Pol.*, 21.15ff. and Sandy's note *ad loc.*

17. RELIGION, WOMEN AND GREEK COLONIZATION

The historian who tries to recover the facts about Greek Colonization in the archaic period is confronted by many problems. Since these are mostly created by lack of source material, they are often either insoluble, or, at best, not amenable to the normal and direct methods of historical argument and analysis. Of such problems a striking and important instance is presented by the women. Baldly put, the question is whether Greek women participated in the foundation of Greek colonies in this period. There is a theory, which is widely held today,[1] that they did not: that the colonies were foundations of Greek men, who ensured the continuance of their new settlements by taking wives from the local native population. It is this thesis that I intend to examine.

It is important at the outset to clarify the limits of what we are discussing. It is not intermarriage as such. We know from plentiful examples that Greeks did not object to intermarriage as a general principle. We have, on the one hand, a small number of well-known historical instances attested in our literary sources. The younger Miltiades, for example, married Hegesipyle, a Thracian princess,[2] and Demosthenes' grandfather, Gylon, married a rich Scythian woman.[3] Secondly, we can confidently conclude that there were numerous similar unions which have not been preserved in our sources, because there is good evidence for mixed communities in the Greek colonial world, which necessarily imply intermarriage on a substantial scale. Herodotus tells us that the people he names Callipidae were 'Helleno-Scythians' (iv.17.1), and that others, called Geloni, were originally from the Greek trading cities (*emporia*) and spoke partly Greek partly Scythian (iv.108.2). We know from Thucydides that there were mixed || settlements of Greeks and 'Pelasgians' on the peninsula of Acte in Chalcidice (iv.109.3-4), and the

[1] See J. Rougé, "La colonisation grecque et les femmes," *Cahiers d'histoire*, xv (1970), pp. 307-317; R. van Compernolle, "Femmes indigènes et colonisateurs," *Modes de contacts et processus de transformation dans les sociétés anciennes* (Collection de l'École française de Rome, 67, Pisa/Rome 1983) 1033-1049.

[2] Hdt., vi.39.2.

[3] Aeschines, iii, 171-2.

Religione e città nel mondo antika, Atti, Centro ricerche e documentazione sull'antichità classica 11 (1981-2), 293-314

evidence of dedications from the sanctuary of the Great Gods at Samothrace seems inescapably to imply that a mixed community existed there too from the second half of the sixth century.[4] These examples suffice, and we need not concern ourselves with others of uncertain date, such as Emporiae in Spain, or of short duration, such as the probable shared community of Greeks and Sicels at Leontini.[5] Nor is it strictly relevant to our theme to adduce cases where non-Greeks married Greek women, such as Scyles and his father,[6] or Sitalces, the Thracian king.[7] On the other hand, it is important to note that, in the fifth century at least, Selinus had arrangements about intermarriage with Segesta,[8] an isolated instance in our evidence, but not necessarily so in fact.

This material is sufficient to show that intermarriage was not uncommon, but such intermarriage is to be distinguished from the practices regarding women at the foundation of a colony. What we are discussing here is whether or not, as a general rule, the women of the newly-established Greek colonies were natives rather than Greeks.

The only Greek foundation where our sources expressly state that the colonists took no women with them, and found their wives from the local population, is Miletus. Herodotus states (i.146.2-3): "Those who set out from the prytaneum of the Athenians, and who consider themselves the most noble of the Ionians, did not take women to the colony, but married Carian women, whose fathers they killed. Because of this killing the women themselves made a law and imposed an oath on themselves (which they handed down to their daughters), never to eat with their husbands, nor to call their own husband by his name, for the reason that the men had killed their fathers, husbands and sons, and then, after doing that, married them." Pausanias' account of the foundation of Miletus (vii. 2.6), which is likely to be || derived, directly or indirectly, from Herodotus, includes the same information.

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[4] See K. Lehmann, *Samothrace II.2, The inscriptions on ceramics* (New York, 1960), pp. 8-19.

[5] Polyaenus, *Strat.*, v.5.1 seems to be in general confirmed by archaeological discoveries; see G. Rizza, "Siculi e Greci sui colli di Leontini," *Cronache di arch. e di storia dell'arte*, i (1962), pp. 3-27; L. Bernabò Brea, *Sicily before the Greeks* (London, 1957) 171f. For Emporiae, see Strabo, iii.159f.; Livy, xxxiv.9.

[6] Hdt., iv.78-80.

[7] Thuc., ii.29.1.

[8] Thuc., vi.6.2.

Although Miletus is strictly irrelevant to our discussion on chronological grounds, the Greeks themselves did not distinguish the colonization of the migratory period from that of archaic times, so it could be argued that what Herodotus describes could be true of the historical period. In any case, since his statement is universally cited as their chief literary support by those who maintain that Greek colonists took native wives, we must examine it closely.

The story can easily be recognized as aetiological. There were certain customs observed by the women of Miletus, which were sufficiently strange to need explanation. This the *aition* provided. The story's aetiological characer is clearly revealed by the statement that the law, which the women had imposed on themselves, was passed on to their daughters, for this provided the necessary link between the existing customs and the events of long ago which were supposed to explain them. An aetiological story cannot be taken as historical evidence for the facts it contains, but it might still be argued that it provides a general testimony to ancient Greek practices. This raises the question whether Herodotus thought the supposed actions of the Ionian settlers normal or abnormal. It seems to me that his words, οὗτοι δὲ οὐ γυναῖκας ἠγάγοντο ἐς τὴν ἀποικίαν, imply clearly enough that in Herodotus' opinion it was normal for colonists to take women with them. Whether or not the details of this interpretation be accepted, the Herodotean story about Miletus is clearly not sufficient evidence for the conclusion that in the Greek colonization of archaic times it was the rule for the colonists to find their wives among the native population.

We may now turn to the colonies of the archaic period itself, and consider those instances where the evidence has been thought by modern scholars to show that the colonists took native wives.

It has often been stated that the colonists of Thasos married Thracian women. The fragmentary beginnings of two lines in the *Monumentum Archilochi*,[9] the first of which contains a quotation from Archilochus himself, were restored and interpreted by Hiller von Gaertringen in this sense. He cited the Herodotean passage just II discussed, and argued that 1.000 colonists went to Thasos, who married Thracian wives.[10] But one only has the look at the remains we possess to see that this interpretation is very bold:

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[9] *IG*, xii Suppl. pp. 212-214 = F. Diehl, *Anth. Lyr. Graec.* (1952), fr. 51; M. Treu, *Archilochos* (Munich, 1959), pp. 52-62.

[10] *Nach. Gött. Gesells. (Altertums.)*, i (1934-6) p. 51.

ὅτι δ' ἀλη[θῆ λέγει ὁ Δημέας]
ὑπὲρ ταύτης τῆς π[ορείας, δηλοῖ ⟨ὁ ποιητὴς⟩ λέγων
τάδε· χειλίους γὰρ ἄν[δρας . . . ἔ-]
πειτα γυναῖκας εἶ[χον . . .].

The appearance of Thracian names among the citizens of Thasos was also interpreted as evidence for intermarriage between the colonists and the Thracians at the time of the colony's foundation,[11] but, as I have shown elsewhere, while these rare occurrences may well attest instances of intermarriage, there is no reason to put those marriages as far back as the beginning of the history of Greek Thasos. They seem likely to be no earlier than the sixth century.[12] On the other hand, we do know of one Greek woman who was involved in the beginnings of this colony, Kleoboia, who first brought the rites of Demeter from Paros to Thasos.[13]

Another colony where the evidence has been similarly interpreted is Cyrene. In his detailed account of the foundation of the colony Herodotus mentions only men as colonists (iv.150ff., especially 153), and the regulations for recruitment in the decree arranging for the foundation preserved in a Cyrenaean inscription of the fourth century are similarly concerned only with men.[14] In addition we know from Pindar (*Pyth.* ix 105-125) and Callimachus (*Hymn to Apollo*, 85ff.) that there were traditions of intermarriage between the Greeks and the Libyan women at Cyrene. Furthermore, the edict of Ptolemy I establishing the constitution of Cyrene laid it down that the sons of Cyrenaean men and Libyan women should have full citizen rights.[15]

At Cyrene too there are a few Libyan names in our onomastic evidence.[16] One of these, Ἀλάζειρ, is the same as that of the king of Barca, whose daughter was the wife of Arcesilas III of Cyrene.[17] This King Alazeir has been taken to be a Libyan, but his daughter is described by Herodotus as a kinswoman of her husband Arcesilas, which means that she was a Battiad. We know that there were

[11] H. Seyrig, "Quatre cultes de Thasos," *BCH*, li (1972) pp. 178-233, especially pp. 217-219; J. Pouilloux, *Etudes Thasiennes*, III, pp. 311-313.

[12] A. J. Graham, "The foundation of Thasos," *BSA*, lxxiii (1978) pp. 61-101, especially pp. 92-3.

[13] Paus., x.28.3; Graham, *op. cit.*, pp. 81-2.

[14] *ML*, 5.

[15] *SEG*, ix.1.1-3.

[16] F. Chamoux, *Cyrène sous la monarchie des Battiades* (Paris, 1953), p. 223.

[17] Hdt., iv.164.4.

Battiads in Barca at the time from the passage Hdt. iv.202.2.[18] So there are two possibilities: either Alazeir was a Libyan, and his daughter's Battiad blood came through her mother, or Alazeir himself was a Battiad with a Libyan name. In the second case, which both Macan[19] and Chamoux[20] regarded as the more probable, the Libyan name may attest intermarriage in an earlier generation, just as the Thracian name of Thucydides' father bore witness to his ancestor Miltiades' marriage to Hegesipyle, daughter of Olorus.[21] Such intermarriage would be readily understandable, since the Battiad founders of Barca had very close relations with the Libyans.[22]

This evidence certainly suggests that intermarriage between Greeks and Libyans was frequent in Cyrenaica, but it does not strictly tell us whether the women of the first settlement at Cyrene were Greeks or Libyans. There is, however, one further interesting piece of information in Herodotus which bears on the question. He tells us (iv. 186) that the women of the Cyrenaeans and those of the Barcaeans observed the same taboos about food as the Libyan women, who in these matters followed the Egyptians. This fact, when combined with the evidence we have mentioned, has convinced many that at Cyrene the Greek colonists found their wives among the native Libyans.[23]

An example of marriage to a native woman at the beginning of a colony's existence is found at Massalia, where, if we follow Justin (xliii.3-4), Protis, one of the leaders of the Phocaean expedition, married the daughter of the local king, or, if we prefer Aristotle (fr. 549, Rose), she was married to Euxenus, whose son, called Protos, was the ancestor of the *genos* Protiadai at Massalia. There is a strong element of the folk-tale in the account of the wedding-feast at which this marriage was || arranged, and the differences between Aristotle's and Justin's accounts are disturbing, but the basis of the story is not incredible in itself. It does not, however, tell us about the rest of the women who began the settlement at Massalia, though it has been assumed that the companions of Protis (or Euxenus) also found their

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[18] Cfr. Macan's note ad loc. (*Herodotus, the fourth, fifth and sixth book*, edited by R. W. Macan, London, 1895).

[19] *Ibid.*

[20] *Op. cit.*, p. 151.

[21] Cfr. Graham, *op. cit.*, 92f.

[22] Hdt., iv.160.1-3.

[23] E.g. Chamoux, *op. cit.*, p. 129; Rougé, *op. cit.*, pp. 315-317; J. Boardman, *The Greeks Overseas, their colonies and trade* (London, 1980), pp. 153-5.

wives among the local inhabitants.[24] But it so happens that in this instance we know of one Greek woman who accompanied the colonists, Aristarche of Ephesus, who introduced the cult of Ephesian Artemis to the new colony of Massalia.[25]

We have been looking at instances which have been taken to support the theory that Greek colonists took native wives, though we have also noted two examples of the contrary, Greek women in colonial foundations. We may turn now to some further examples of the latter phenomenon.

At Locri Epizephyrii, if we could believe the Aristotelian tradition about the foundation of the colony, we should have a very clear case of Greek women who participated in an archaic colonial enterprise. For this tradition stated that Locri Epizephyrii was founded by distinguished Locrian women together with the slaves, with whom they had formed relationships during the long absence of their husbands, who were fighting on the side of the Spartans in the Messenian War.[26] However, not only was this tradition effectively criticised in antiquity, it has been shown by modern scholars to be entirely unconvincing.[27] We cannot, therefore, gain anything from it for our purposes.

Nor can we use the tale about the foundation of Taras, in which the *oikistes*, Phalanthus, was accompanied by his wife, Aethra,[28] as evidence for the presence of Greek women at a colonial foundation. The story is based on a riddling oracle, that Phalanthus would possess land and city, when he perceived rain falling from a clear sky (αἴθρα). After long sufferings, his wife's tears fell on his head, and Phalanthus realised that the oracle was fulfilled. Oracles of this kind are a || notoriously concocted element in our traditions about colonial foundations,[29] and this one must clearly be rejected as unhistorical.

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[24] Rougé, *op. cit.*, p. 314.

[25] Strabo, iv.179.

[26] Polybius xii.5-11, especially 5,6,8; 6b.10. Cfr. F. W. Walbank, *A Historical Commentary on Polybius*, II (Oxford, 1967), 330f.

[27] Notably by R. van Compernolle, "Le tradizioni sulla fondazione e sulla storia arcaica di Locri Epizefiri e la propaganda politica alla fine del V e IV secolo avanti Cristo," *Annali d. scuol. norm. sup. Pisa*, ser. III, vi.1 (1976), pp. 329-400. For earlier criticisms, see Walbank, *ibid.*

[28] Paus., x.10.6-8.

[29] See A. S. Pease, "Notes on the Delphic Oracle and Greek Colonization," *CP*, xii (1917), pp. 1-20. Cfr. H. W. Parke and D. E. W. Wormell, *The Delphic Oracle* (Oxford, 1956), 1.49-81.

The presence of Aethra, which is an essential part of the story of the oracle therefore falls with it.

It is just possible, however, that the wife of an *oikistes* has been discovered by archaeology. According to Thucydides (vi.4.1), Lamis, the *oikistes* of Megara Hyblaea, died when the colonists were temporarily settled on the peninsula of Thapsus. Among the many burials of Bronze Age date on this peninsula, Orsi found a single later grave at a higher level. This contained two skeletons, two Corinthian cups of Late Geometric date and a pair of bronze tweezers.[30] Since this isolated burial is of the right date, it has regularly been recognized as the grave of Lamis.[31] But, if that is so, then it seems probable that the second body was that of his wife. Where one finds double depositions in Greek burials, it is a normal assumption that we have to do with man and wife.[32] Obviously there are many unknowns here. The bones have long since disappeared and there is no record that their sex was ever established. Tweezers were normally a man's implement in the Bronze Age, when they are common.[33] In the Iron Age they are very rare,[34] and it would be hazardous to decide which sex they point to. It may also seem far-fetched to imagine Lamis and his wife dying together sufficiently close in time for a joint burial in this very simple grave. Should we not expect that the colonists would have re-buried their founder in a prominent position in their permanent home? These considerations perhaps make it more prudent to give up the idea that this is Lamis's grave, and conclude merely that it dates to the shortlived Megarian occupation of Thapsus.[35] But we should ll note that those who see it as the grave of Lamis should accept as a probable consequence that his wife accompanied him.

There is one act of colonization in the archaic period, where we are expressly told that women and children took part as well as men.

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[30] P. Orsi, 'Thapsos', *Mon. Ant.*, vi (1895), 103f., plate IV, no. 16; cfr. G. Vallet and F. Villard, "Les dates de la fondation de Mégara Hyblaea et de Syracuse," *BCH*, lxxvi (1952), p. 337.

[31] E.g. T. J. Dunbabin, *The Western Greeks* (Oxford, 1948), p. 19.

[32] See D. Kurtz and J. Boardman, *Greek Burial Customs* (London, 1971), p. 194; cfr. M. Cebeillac-Gervasoni, "Les nécropoles de Mégara Hyblea," *Kokalos*, xxi (1975), p. 34.

[33] See G. Marinatos, *Archaeologia Homerica*, I.B.34ff.; cfr. *Reallexicon der Vorgeschichte*, s.v. Pinzette (Götze).

[34] Marinatos, *ibid.*

[35] As Vallet and Villard, *ibid.*

This is the Phocaean expedition which shared briefly in the settlement at Alalia in Corsica, and later established itself permanently at Elea.[36] However, since this was in intention a complete evacuation of the mother city in face of the Persian threat, it is obviously, in degree at least, an exception in the history of Greek colonization. So what it tells us about the participation of women has been rejected as exceptional by those who think that the general rule was for men to go alone and find their wives among their non-Greek neighbours.[37]

This rapid survey of the evidence would not be complete without a mention of one brilliant hypothesis, which was recently put forward on the basis of our archaeological evidence. One of the most enigmatic facts about the Greek colonists of Sicily and Italy is that they took over metal personal ornaments, especially *fibulae*, of Italic type, and abandoned the types of *fibulae* current in Greece. So there was thereafter a clear dichotomy between the Italiot and Siceliot Greeks on the one hand, and those of the homeland on the other. *Fibulae* of the homeland type are virtually not found in the Greek sites of the West, while in Greece itself the Italic types were not worn, for though they are occasionally found as dedications, they do not occur in graves.[38]

Dr. G. Buchner's excavations at Pithecusa have shown that the Greek colonists were manufacturing these *fibulae* of Italic type in the second half of the eighth century,[39] so we now know that the Greek colonists were not only using but also making *fibulae* of Italic type from the early days of their settlement in the West.

Buchner has also offered an explanation of the phenomenon. He says that the initiative for adopting these Italic personal ornaments cannot have come from the men, since the majority of these ornaments were for women. So it can only have been the women who imposed it, and that shows that at least the majority of the wives of the || colonists were not Greek, but native women who wished to keep their customary ornaments.[40]

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[36] Hdt., i.164.3-167.

[37] As by Rougé, *op. cit.*, p. 312.

[38] See Chr. Blinkenberg, *Fibules grecques et orientales* (Copenhagen, 1926) pp. 25-6.

[39] G. Buchner, "Nuovi aspetti e problemi posti dagli scavi di Pithecusa con particolari considerazioni sulle oreficeri di stile orientalizzante antico," *Cahiers du Centre Jean Bérard*, ii (Naples, 1975) pp. 59-86.

[40] *Ibid.*, p. 79.

Attractive and subtle though this argument is, it is open to various objections. From the same period we have a number of famous 'princely' tombs at Cyme. The grave goods include large numbers of these metal ornaments and *fibulae* of Italic type. Yet most of the graves were of men, and these men were patently Greek nobles.[41] If they possessed and wore these objects in abundance, they can hardly have seen them as specific either to women or to the native population.

Secondly, at the Fusco cemetery in Syracuse, where many early graves have been excavated, most of the women are buried with the two pins at the shoulders, which indicate that they wore the standard (Doric) peplos. These are surely Greek women in their traditional Greek dress. Yet there are quite large numbers of *fibulae* of Italic type in these graves. They occur either with juveniles or, in a small proportion of the graves, with adults, serving as substitutes for the commoner pins.[42] These facts are difficult to harmonize with Buchner's hypothesis about native women. It seems clear that the manufacture of these objects in the West was quickly so organized that even for Greek men, or for a population that certainly contained Greek women, the only *fibulae* and other metal personal ornaments available were those of Italic type. However we explain this fact, it does not seem likely to have arisen from the desire of native women in the Greek colonies to keep their habitual ornaments.

This survey of the direct evidence reveals clearly enough that it is insufficient to answer the question we are discussing. Women, whether Greek or native, are largely absent from the serious accounts we possess of colonization in the archaic period. An argument from silence, viz. that, because women colonists are not mentioned, Greek women did not participate, is not permissible. We can tell that quite definitely from an examination of colonization in the fifth century and our records of it, both literary and in documents. Thucydides mentions several fifth-century acts of colonization: the reinforcement of Epidamnus by Corinth (i.27.1); the colonization of Amphipolis by Athens (i.100.3; iv.102.2-3); and that of Heraclea in Trachis by || Sparta (iii.92.5). He invariably uses the masculine gender in his description of the colonists. We find the same thing in inscriptions. The Athenian

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[41] *Ibid.*, 74ff.; C. Albore-Livadie, Remarques sur un groupe de tombes de Cumes, *Cahiers du Centre Jean Bérard*, ii (Naples, 1975), pp. 53-8.

[42] See H. L. Lorimer, *Homer and the Monuments* (London, 1950), p. 338.

decree establishing a colony at Brea, and the Locrian decree arranging for the reinforcement of Naupactus, both mention male colonists only.[43] Yet we know quite definitely from the events at Thurii that at this period Greek women did participate in colonial ventures.

The Athenians sent colonists to Thurii in 444/3 in order to reinforce the Sybarites, who had failed in their attempt to re-found their city.[44] Disputes quickly arose between the original Sybarites and the newcomers. One of the causes of friction was that the Sybarites thought that their wives should have preference over the wives of the new colonists in sacrifices to the gods.[45] So the colonists of Thurii brought wives with them. We must therefore conclude that the absence of any mention of women in Thucydides' accounts of contemporary colonization, or in contemporary documents arranging for colonization, does not mean that women did not participate. It merely means that it was thought unnecessary to mention them. Just as Thucydides does not think it necessary to make Nicias mention women, when he told his army in Sicily, "You yourselves immediately make a city, wherever you settle," and asserted Ἄνδρες γὰρ πόλις καὶ οὐ τειχῆ, οὐδὲ νῆες ἀνδρῶν κεναί (vii.77.4 and 7).

So the absence of women in our sources, or even an emphasis on men, such as Herodotus' in his account of the colonization of Cyrene, has no evidential value. Without this negative argument, there is simply insufficient direct evidence, for any archaic Greek colony, to prove definitely that the colonists took native wives. This is true of Cyrene, where the case is strongest, and thus *a fortiori* for all the rest.

On the other hand, we have positive evidence for the presence of two Greek women in archaic colonial ventures, Kleoboia at Thasos and Aristarche of Ephesus at Massalia. Such women have been judged exceptions,[46] and they certainly are exceptional in one sense. For, apart from the *oikistes* or *oikistai*, it is very rare indeed for our || evidence to preserve any names, of either men or women, who took part in archaic colonial foundations.[47] Thus the rarity of these two

[43] *ML*, 49, 20.
[44] Diod., xii.10f. See V. Ehrenberg, "The foundation of Thurii," *AJP* lxix (1948), 149ff.
[45] Diod., xii.11.1.
[46] See Rougé, *op. cit.*, 313.
[47] Three companions of Pentathlus are named in the passage Diod. v.9, but they

women in our record cannot by itself establish that they were also exceptions in fact.

It is interesting to observe what they did and what they were. Kleoboia was said to be the first to bring the rites of Demeter from Paros to Thasos, and was depicted in the painting by Polygnotus at Delphi as having on her knees a chest of the kind that was customarily made for Demeter.[48] As for what she was, in Polygnotus' picture she was going to Hades in Charon's boat, still a virgin, and altogether I do not think it would be hazardous to conclude that she was the first priestess of Demeter at Thasos. Aristarche of Massalia was a distinguished woman of Ephesus, who was instructed by the goddess Artemis in a dream to accompany the Phocaean colonists to Massalia, and establish a new branch of the cult of Ephesian Artemis there[49] So she took with her the necessary means for such a transference of cult,[50] and, when the colony was established at Massalia, the new sanctuary was duly built. Aristarche was given the oustanding honour of being appointed priestess.

We may have some confidence in the historical reality of these two women, because the Greeks seem regularly to have preserved the names of people who introduced new cults, as, for instance, Pegasos of Eleutherai, who introduced the cult of Dionysus Eleutherios ||

may well have been συγκτίσται, as Herodotus calls the four companions of Dorieus, whom he names at v.46.1. The ancestor of Gelon who joined in the colonization of Gela is not named by Herodotus at vii.153.1. It has been regularly assumed that his name was Deinomenes, but there are serious objections to that assumption; see J. P. Kesteman, "Les ancêtres de Gélon," *Ant. Class.*, xxxix (1970), pp. 395-413 (with earlier literature).

[48] Paus., x.28.3; for a detailed discussion, see Graham, *op. cit.*, pp. 81-2.

[49] Strabo, iv.179.

[50] The phrase ἀφίδρυμά τι τῶν ἱερῶν λαβοῦσῃ is misunderstood by translators, as Jones (Loeb) "taking with her a certain reproduction which was among the sacred images" or Lasserre (Budé) "prenant avec elle un modèle réduit du sanctuaire," and by interpreters, as Dirkzwager, *Strabo über Gallia Narbonensis* (Leiden, 1975), who still relies on the inadequate and misleading article on ἀφίδρυμα in Liddell and Scott's lexicon. For the true meaning, see J. Brunel, "Un sens méconnu du mot ΑΦΙΔΡΥΜΑ" *Rev. de Phil.*, lxxix (1953), pp. 21-33, who makes it probable that the ἀφίδρυμα actually consisted of part of the remains of sacrifices from the altar, with which it was possible to consecrate a valid new altar for a new branch of the cult in question.

L. Robert opposed Brunel's contention that ἀφίδρυμα never means "a copy of a statue," see *Hellenica*, xiii (1965) pp. 120-125. In this he was probably right, but he did not pay due regard to the chief arguments in favour of Brunel's main thesis, and, in particular, he significantly failed to mention instances which provided its strongest support.

at Athens,[51] or were important in the history of cult, such as Telines, Gelon's ancestor, whose memory was preserved at Gela.[52]

It is my belief that these two women, Kleoboia and Aristarche, provide us with a clue, which we only have to follow to find the answer to our problem about women in Greek colonization. What they require us to do is to approach the problem indirectly, by turning our attention to the religion of the *polis*, and, in particular, to the role of women in that religion.

It was normal in Greece for female deities to be served by female ministrants, and male deities by males, though there are exceptions to this rule in both directions.[53] The role of the priestess, as of the priest, was to supervise and conduct the rituals of the cult, notably prayers and offerings, and to protect and control the altar, temple and sacred property of the deity.[54] As part of her duties the priestess commonly was alone allowed to open the house of the god or goddess, or at least the most holy parts of it, and thus ritual acts could not normally be performed without her presence.[55] This is already clear from the famous passage of the Iliad where Theano, wife of Antenor, who has been chosen priestess of Athena by the people, is needed to open the doors of the temple in order that the goddess may be supplicated (vi.297ff.).

In the speech against Neaera ([Dem.] lix.72-8) we have a good brief picture of the activities of the wife of the Archon Basileus at Athens, who was in this instance performing priestly duties. She had to lead the celebration of secret rites of Dionysus at the Anthesteria

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[51] Paus., i.2.5; 38.8 Cfr. H. W. Parke, *Festivals of the Athenians* (London, 1977), 125f.

[52] Hdt., vii.153; cfr. Kesteman, *op. cit.*

[53] See L. R. Farnell, *Cults of the Greek States* (Oxford, 1896), i.320; "Sociological hypotheses concerning the position of women in ancient religion," *ARW*, vii (1904) pp. 70-94.

[54] I know of no modern, comprehensive treatment of Greek priestesses. J. V. Adrian, *Die Priesterinnen der Griechen* (Frankfurt am Main, 1822), is inevitably outdated. There is useful material in: L. F. A. Maury, *Histoire des religions de la Grèce antique* (Paris 1857-9) ii.381-431; K. F. Hermann, *Lehrbuch der gottesdienstlichen Alterthümer der Griechen* (Heidelberg 1858) 204-226; J. Martha, *Les Sacerdoces Athéniennes* (Paris 1882); P. Stengel, *Die griechischen Kultusaltertümer* (Munich 1920), chapter 2; RE, s.v. Hiereis; Douglas D. Feaver, "Historical development in the priesthoods of Athens," *Yale Class. Stud.* xv, (1957), pp. 123-58; Kevin Clinton, "The sacred officials of the Eleusinian Mysteries," *Trans. Amer. Philos. Soc.*, lxiv, pt. 3 (1974); Walter Burkert, *Griechische Religion der archaischen und klassischen Epoche* (Stuttgart 1977) 157-163.

[55] E.g. IG, II² 1177 = F. Sokolowsky, *Lois sacrées des cités grecques* (Paris 1969), 36.

and make sacrifices that could not be named. In addition she || had to administer an oath to the fourteen *Gerairai*, themselves female ministrants of the god, who also performed sacrifices.[56] For this function it was necessary for her to speak correctly the necessary words and formulas, which we know consisted of more than the oath which our texts preserve, even if that is authentic.[57] In that the *Gerairai* are to swear that they will conduct appropriately the Theoinia and Iobaccheia of Dionysus. Finally, she had to perform the ceremony of being 'married' to the god.[58]

The two greatest priestesses at Athens were those of Athena Polias and of Demeter and Kore at Eleusis. The importance to the community of these two ancient cults was reflected in the responsibilities of these two priestesses, and in the honour conferred on them by their offices.[59] They were people of great distinction in Athenian society, who could act with authority and independence. One priestess of Athena Polias forbade the Spartan king Cleomenes to enter the temple of the goddess,[60] and Theano, priestess of Demeter and Kore at Eleusis, refused to obey, when ordered by the people of Athens to curse those convicted of parodying the Mysteries.[61]

The distinction of priestesses is well attested in many other parts of the Greek world. Herodotus bears witness to the antiquity and prestige of the office of priestess at Dodona, and shows that the names of the priestesses were preserved (ii.55.3). The same was true of the priestess of Hera at Argos. Hellanicus was able to publish a list going back to mythical times, and statues of the priestesses were erected in the precinct.[62] Plutarch tells us that Lycurgus decreed that no one should have their names inscribed on their tombs except men who died in war and women in sacred office (*Lyc.* xxvii.2). One is reminded of the same writer's statement that in some parts of Greece the the standing of the priesthood was equivalent to that of a king (*Quaest. Rom.* 113), or of Homer's priest of Zeus, who was

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[56] See H. W. Parke, *op. cit.* 111f.

[57] Note the words ὅσα οἷόν τ' ἐστὶ ἀκούειν ([Dem.] lix.78).

[58] See also Arist. *Ath. Pol.* 3.5.

[59] An early testimony to this is the inscription of Lysistrata, priestess of Demeter and Kore at Eleusis, SEG x.321; see Clinton, *op. cit.* 69.

[60] Hdt. v.72.3.

[61] Plut. *Alc.* xxii.4. For the proof that Theano was indeed the priestess at Eleusis, see Clinton, *op. cit.* 16.

[62] See Jacoby, commentary to FGH 4 frs. 74-84.

honoured as a god by the people (*Il.* xvi.605). His priestess of Athena at Troy, || Theano, whom we have already mentioned, was the wife of a leading man, and, in later authorities, sister of the king.[63]

We should not be surprised, therefore, that the qualifications for occupying these distinguished positions were strict. At Athens we know that no metic or foreigner could be a priest, and even a foreigner who had been given Athenian citizenship was debarred, though his progeny were eligible if their mother was an Athenian citizen.[64] The wife of the Archon Basileus, some of whose priestly duties we have mentioned, had to be a citizen and to have come to her husband a virgin.[65] Only women of the noble and ancient clan of the Eteoboutadae were eligible to be priestesses of Athena Polias, and the office was held for life.[66] The priestesses of Demeter and Kore at Eleusis similarly held office for life, and appointment was restricted to women of the clan Philleidai and, as we know from an inscription, at least one other *genos*.[67]

We hear of priestesses in other Greek cities, who were chosen for their distinguished families,[68] but such a requirement was not invariable. The Pythia herself could come from a humble family, so long as she was of legitimate, citizen, birth and morally correct life, and observed the requirement of chastity.[69] But if distinguished birth was not always a necessity, our evidence suggests that citizen birth always was. At Halicarnassus the requirements for the priestess of Artemis Pergaea were, according to a law of the third century BC, that she be a citizen born of citizens for three generations, in both her father's and her mother's line.[70]

Given our fragmentary evidence, it would be difficult to determine for any Greek city how many priestesses there were. We think primarily of the great goddesses, Hera, Athena, Artemis, Aphrodite, Demeter and Persephone, but it is very unlikely that they would provide an exhaustive list. There were also lesser deities of special concern to women, such as Eileithyia, and local godheads, who might

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[63] RE s.v. Theano 2.
[64] [Dem.] lix.92, 106; Dem. *Euboul.* 48.
[65] [Dem.] lix.75.
[66] Harpocration, s.v. 'Ετεοβουτάδαι.
[67] See IG II^2 2945 and Clinton, *op. cit.* 74.
[68] E.g. Paus. vii.27.3.
[69] Plut. *de Pyth. orac.* 405 C-D; cfr. Parke and Wormell, *op. cit.* i.35-41.
[70] $Syll.^3$ 1015 = F. Sokolowski, *Lois sacrées de l'Asie Mineure* (Paris 1955), 73.

be served by priestesses.[71] At Athens, admittedly one of the greatest cities, apart from the great state cults, there were local cults in the demes as well,[72] so that the total number of priestesses must have been considerable.

Even when we have tried to grasp the number of priestesses, we have by no means completed the list of women who served the gods and goddesses. There are those, like the two Hierophantides at Eleusis, whose office and duties are very similar to those of priestesses, and carry similar distinction.[73] At Delphi there were women whose duty it was to maintain the sacred fire. They had to meet the same requirements as to purity as the Pythia herself.[74] We should envisage many such women performing similar services in Greek cults. At Sicyon, for example, entry to the temple of Aphrodite was permitted only to the virgin priestess, who served for a year and was called the Bathbearer, *loutrophoros*, and to the *neokoros*, keeper of the temple, who was no longer permitted to have relations with a man.[75] At Olympia, the *adyton* where the image of Sosipolis was kept could be entered only by the old woman who was responsible for the shrine, who was required to live in chastity.[76] In Boeotia, fire was placed daily on the altar of Iodama by a woman who was required at the same time to say three times the words "Iodama lives and asks for fire." According to the local myth, Iodama herself had been a priestess, who entered the temple at night and saw Athena; whereupon she was turned to stone by the Gorgoneion.[77]

Even this rich and varied picture of women's service in Greek religion is put into the shade, however, when we widen the angle of our enquiry to embrace women's cults in general. There were many cults and festivals in Greece which either belonged to women or offered women a special role. These were especially ancient fertility rites, like the Thesmophoria, a cult of the corn goddess, Demeter, found all over the Greek world.[78] From such cults men were

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[71] E.g. Martha, *op. cit.* 162 no. 83; Paus. vi.20.2.
[72] See Parke, *op. cit.* 175ff.; Sokolowski, *Lois sacrées des cités grecques*, 36 (= IG II^2 1177).
[73] See Clinton, *op. cit.* 86-8.
[74] Plut. *Numa* ix.5; Parke and Wormell, *op. cit.* 35f.
[75] Paus. ii.10.4.
[76] Paus. vi.20.2.
[77] Paus. ix.34.1-2.
[78] See, e.g., Hdt. ii.171.2-3; vi.16.2; M. P. Nilsson, *Griechische Feste von religiösischer Bedeutung, mit Ausschluss der attischen* (Leipzig 1906) 313ff.; Parke, *op. cit.* 82-8.

rigorously excluded; all the officials were women. The age-old traditional rituals would consist of processions, dances, hymns, prayers, sacrifices || and the displays of sacred objects.[79] In Aristophanes' *Thesmophoriazousai* the chorus of women sing two hymns, one invoking Artemis, Hera, Hermes, Pan and the Nymphs, and Dionysus, the other Athena, Demeter and Persephone.[80] We may think of these deities as specially worshipped by women at Athens.

Our attention is inevitably concentrated on Athens by the evidence available to us. This evidence reveals many women's cults apart from the Thesmophoria. For instance, we have the Anthesteria, as a whole a great wine festival but with a women's fertility rite at its core;[81] the Kallynteria and the Plynteria, festivals of cleansing and beautifying the goddess Athena, where the close contact between the women and the leading deity of the city was manifested;[82] the Chalkeia, when the warp for the new *peplos* for Athena was set up on the loom;[83] the Haloa, a secret fertility rite of women performed at Eleusis in honour of Demeter and Dionysus;[84] the Skiraphoria, an apparently complex festival about which scholars disagree, but which certainly contained a women's fertility rite sacred to Demeter and Dionysus, from which men were excluded;[85] and, finally, the Arrhephoria, when four young girls went down by night from the shrine of Athena Polias to a sanctuary of Aphrodite, carrying unknown objects, which they left below, and then returned bearing something else which also could not be mentioned.[86]

Although our evidence compels us to concentrate on Athens, we know enough about women's cults in the rest of Greece to be confident that Athens was not atypical. For example, the Thyiades of Athens, women who celebrated rites of Dionysus every other year high on Mount Parnassus, were joined in this worship by similar women from Delphi.[87] We may confidently accept that the special

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79 *Ibid.*
80 Aristoph. *Thesm.* 969ff., 1136ff.; cf. Parke, *op. cit.* 85.
81 Parke, 107-113.
82 Parke, 152-155.
83 Parke, 38.
84 Parke, 98-100.
85 Parke, 156-162.
86 Parke, 141-143. For interesting suggestions about this festival, see M. Torelli, 'I culti di Locri', *Locri Epizefirri. Atti del sedicesimo convegno di studi sulla Magna Grecia* (Napoli, 1977), p. 178.
87 Paus. x.4.3; 32-7.

relation to Dionysus shown by these women was universal in the Greek world, just as the great women's cult of Demeter and Persephone. There were || certainly also plenty of other women's cults all over the Greek world. In Tegea even Ares had a cult celebrated by women from which men were excluded. As a result he bore the epithet "feaster of women," γυναικοθοίνας.[88]

Let us look, therefore, at some of the roles of women in cults at Athens. There is an oft-quoted passage of Aristophanes, *Lys.* 641ff., in which the chorus express the pride of an Athenian woman in her youthful sacral activity. At seven years old she had served as *arrhephoros*, one of the four young girls who lived for a year on the acropolis and then performed the mysterious rite mentioned above. At ten she had fulfilled the role of *aletris* for the Archegetis. This task was to grind sacred meal for offerings to Athena.[89] Next she served as one of the little bears, *arktoi*, at the sanctuary of Artemis Brauronia, wearing a saffron robe.[90] Finally, she achieved the honourable position of *kanephoros*, one of the distinguished young maidens, who carried sacrificial baskets in procession. We know that there were one hundred *kanephoroi*.[91]

Aristophanes' chorus did not exhaust the possible religious activities of Athenian girls. We know that the children who served at the altar at Eleusis, the παῖδες ἀφ' Ἑστίας, could be either boys or girls. They had to be citizens and were chosen by lot from a pre-selected list.[92] But when she grew up a whole series of further religious services were open to the adult Athenian woman.

We have already mentioned the *Gerairai*, the revered ones, fourteen in number, who performed sacrifices for Dionysus at the Anthesteria and carried out the festivals called Theoinia and Iobaccheia. For the celebration of the local Thesmophoria in Attica we learn that two women were appointed as presiding officials, who had to perform the customary duties and were in charge of the festival.[93] They were chosen from the wives of demesmen, and fulfilment of this task is used by Isaeus as proof of a woman's citizenship and

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[88] Paus. viii.48.4-6. For a possible explanation, see Nilsson, *op. cit.* 403, who thinks that the rite might have been designed to ensure the men success in war.

[89] See Parke, *op. cit.* 140f.

[90] See Parke, 139f.

[91] Arist. *Ath. Pol.* 18.2.

[92] See Clinton, *op. cit.* 98ff.

[93] See Isaeus, viii.19-20: ποιεῖν τὰ νομιζόμενα . . . συνιεροποιεῖν . . . κυρίαν . . . ἱερῶν.

legal marriage. At the Thesmophoria women called *Antletriai* had the task of bringing up the remains of the pigs, which had previously been deposited || in caves, in order to make the traditional offerings.[94] At the Oschophoria an unspecified number of women "bore the banquet" and were called *Deipnophoroi*.[95] Finally, we may mention the *Ergastinai*, at least one hundred in number, who made the new *peplos* for Athena.[96] These were, like the *kanephoroi*, noble maidens.

This last activity can be paralleled in other cities. At Amyclae the women wove a robe for Apollo,[97] and at Olympia, every fourth year the Sixteen women of Elis wove a robe for Hera.[98] From Locri Epizephyrii we have many representations of the women's *peplophoria* on the famous *pinakes*.[98a] Here we may again assume a very widespread cult practice performed by women for a wide variety of Greek deities.[99]

An important question regarding these cults were the qualifications for admission. We know that it was common not to admit foreigners to Greek cults, and our evidence shows this to have been true also of women's cults.[100] The Mysteries at Eleusis were famous for admitting all ranks of society, including slaves, but they did not admit non-Greeks, or at least those who could not speak Greek.[101]

This brief selection of material concerning the role of women in Greek religion shows that it is indeed true—to cite a recent book about women in ancient Greece—that "Religion in Greece was about the only institution of Greek society that was suffused with women from top to bottom, from the priestesses and prophetesses to Sostratus' mother, who goes in circles around the whole deme sacrificing."[102] The questions we must now ask are, firstly, was the situation the

[94] See Parke, *op. cit.* 82f.

[95] See Parke, 77f.

[96] See Parke, 43.

[97] Paus. iii.16.2.

[98] Paus. v.16.2.

[98a] See Helmuth Prücker, *Die Lokrischen Tonreliefs* (Mainz 1968), 42ff.; Torelli, *op. cit.*, 165.

[99] Cfr. Ines Jucker, 'Frauenfest in Korinth', *Antike Kunst* vi (1963) 47-611, especially 56f.

[100] E.g. the Lenaia, see Parke, *op. cit.* 104f.; the Anthesteria, see [Dem.] lix.73; the Thesmophoria, Isaeus vi.49-50.

[101] Isoc. iv (*Panegyricus*) 157; Libanius, *Decl.* xiii.20 (Teubner); Origen, *Cels.* iii.59; cfr. Theon Smyrnaeus, *Math.* p. 14 (Hiller).

[102] David M. Schaps, *Economic rights of women in ancient Greece* (Edinburgh 1979) 73 (referring at the end to Menander, *Dysc.* 262-3).

same in the religion of Greek colonies, and secondly, if so, what does that imply about the women of those colonies. ||

The overriding importance of religion in general in Greek communities does not require demonstration. It is charmingly illustrated when Plato in the *Laws* uses colonization as a metaphor for a new marriage, the purposes of which he describes as passing on life and always cultivating the gods according to custom (776b). Since religion was important so was the priesthood. Aristotle includes it among the primary needs of a state (*Pol.* vii 1328 b11), and Plato lays down rules in the *Laws* for establishing priests and priestesses in his new colony (759 a-c). First, he says, do not disturb hereditary priesthoods, both male and female; for newly established cults choose people who are over sixty, both male and female, and who are sound, of legitimate birth, from pure households, and have had no contact with murder or any other offence against the gods.

With these general considerations we may combine the indications that in archaic Greek colonization religion was indeed a primary concern of the founders. Homer gives a brief list of the duties of a city-founder in the Odyssey (vi.711), and one of these is the building of temples of the gods. Thucydides tells us that at Naxos in Sicily, the first Greek settlement on the island, the colonists built an altar of Apollo Archegetes (vi.3.1). Battus is praised by Pindar for establishing splendid sanctuaries of the gods at Cyrene (*Pyth.* v.89). Herodotus shows us that the whole community life of the Greeks at Naucratis was organized round the sanctuaries of the gods (ii.178). Recent archaeological discoveries at Gravisca show that the Greek traders who visited the Etruscan port set up cults of their gods there.[103]

In addition to such specific instances, our knowledge of the cults of Greek colonies shows that it was standard practice for the colonists to establish in their new cities the cults that were maintained in their mother cities.[104] It was once believed that there was widespread adoption of native cults by Greek colonists, but there is no good evi-

[103] J.-P. Morel, "L'expansion phocéenne en occident: dix années de recherches (1966-1975)," BCH xcix (1975) 862f.; M. Torelli, 'Il santuario di Hera a Gravisca', *Parola del Passato* xxvi (1971) 44-67; 'Il santuario greco di Gravisca', *Parola del Passato* xxxiii 1977, 398-458.

[104] This was established for the Ionian and Megarian colonies in careful studies by F. Bilabel (*Die ionische Kolonisation, Philologus*, Supplementband, xiv, Leipzig, 1920) and K. Hanell (*Megarische Studien*, Lund, 1934). It may be taken as universally true.

dence for such a practice in the archaic period.[105] As part of his || argument for the importance of women in the foundation of Locri Epizephyrii, Polybius states that the colonists adopted the rites of the Sicels who had previously inhabited the site, but, whereas the Sicels had a boy lead the processions, the Locrians made the *phialephoros* a girl (xii.5.10.f.). This part of Polybius' material comes from the oral tradition of second-century Locri, which has been shown to be quite unreliable,[106] and, in any case, female *phialephoroi* are attested elsewhere in the Greek world.[107]

In general, therefore, we may take it as certain that the religion of the colonies was the same as that of the cities of old Greece and just as important to them. We also have in detail clear indications of the importance of women's cults in Greek colonies, especially in the West. Cicero tells us that at Catana the rites of Demeter were performed by women and virgins, and the priestesses and elderly women of high birth were in control of the cult (*Verr.* II.iv.99). From the same author we learn that the cult of Demeter, Dionysus and Kore at Rome was introduced from the Greek world, and the priestesses were always Greek women, usually obtained from Naples or Velia, who were made Roman citizens (*Pro Balbo* 24). The importance of the cult of Demeter and Kore generally in Sicily is mirrored in the belief that the Rape of Persephone took place there.[108] Another western cult where women are clearly attested as important was that of Hera Lacinia.[109] From Epizephyrian Locri we possess actual representations of priestesses on the *pinakes*, which are richly revealing of women's cults.[109a] The knowledge that women will have had important roles to play in the religion of Greek colonies at all levels from priestess to common worshippers carries with it inescapable consequences about these women. We have seen the duties, respon-

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[105] See, for example, P. Devambez, "Questions Thasiennes," *Journal des Savants*, 1955, pp. 28-40, pp. 73-91, and G. Vallet, "La cité et son territoire dans les colonies grecques d'occident," *La Città e il suo territorio. Atti del settimo convegno di studi sulla Magna Grecia* (Naples, 1968), 81ff. For an example of the older view, see, e.g., J. Bérard, *L'expansion et la colonisation grecques jusqu'aux guerres médiques* (Paris, 1960), 88f.

[106] R. van Compernolle, *op. cit.*, (n. 27 above), especially 334ff.

[107] S. Pembroke, "Locres et Tarente: le rôle des femmes dans la fondation de deux colonies grecques," *Annales* (*ESC*), xxv (1970), p. 1257.

[108] See, e.g., Diod., v.2.3ff.

[109] See, *Anth. Pal.*, vi.265; cfr. van Compernolle, *op. cit.*, 358ff.

[109a] See Prückner, *op. cit.*, 20 with Abb. 2, 42f., with Abb. 5. For the identification, see the convincing discussion of Torelli, "I culti di Locri," 161f.

sibilities, honours and qualifications of priestesses. In view of these we must surely conclude that it is inconceivable that Greeks would ever imagine that the office of priestess could be filled by some native woman. It follows that all properly established Greek colonies needed a minimum of suitably qualified || Greek women to serve as priestesses. That means distinguished, mature, women, in many instances, no doubt, members of the one clan or family eligible; women who possessed the traditional knowledge to perform the traditional rites in the traditional way which would alone please the deity concerned. Thus Kleoboia and Aristarche, far from being exceptional in fact, are the sole instances in our record of a practice in Greek colonization that must have been universal.

However, the colonists would not satisfy the demands of the gods merely by bringing in the necessary priestesses. It is equally inconceivable to imagine the performance of the rituals of Greek women's cults by a population of largely native women. In fact, we only have to think of Greek women's cults, with their exclusion of men, to realise that the idea of Greek men setting out alone to establish a new Greek community without any Greek women is absurd. Would the men presume to teach the local native women the secret rituals of women's cults from which they had been strictly excluded? How could native women, who could not even speak Greek, take an appropriate part in the hymns and prayers dear to the gods and goddesses of the Greeks?

Thus, by approaching the problem of women in Greek colonization through the consideration of Greek religion, we are led inescapably to the conclusion that the great majority of women in Greek colonies must have been from the beginning Greek. We must therefore recognize that, even though there may have been plenty of intermarriage in the history of Greek colonization, the notion that Greek colonies were as a rule established by men only, who took native wives, must be abandoned.

It is probable that one might also reach this conclusion by considering other contributions of women in an ancient Greek community. Even if we leave out the economic role,[110] on the grounds that that might have been fulfilled by non-Greek women, they made

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[110] See P. Herfst, *Le travail de la femme dans la Grèce ancienne* (Utrecht, 1922, repr. Arno Press, New York, 1979).

other contributions which were indispensable. For instance, Deubner showed in a very penetrating enquiry that the *ololuge*, the women's shriek, was a ritual act at important moments such as a birth, or a sacrifice, not to mention in the ceremony of mourning.[111] More recently, || Jucker demonstrated that there was ritual significance in the important women's roles of spinning and weaving.[112] These considerations point in the same direction as the worship of the gods: Greek women were essential to a well-ordered Greek community. We may recall Aristotle's statement, that the education of women is bound to be important, since women make up a half of the free population (*Pol.* I.1260 b 13ff.).

Only if Greek colonial foundations were semi-piratical ventures could one envisage the currently fashionable view about the origins of the women. There were such semi-piratical ventures, it is true,[113] but the vast majority of colonies in the archaic period were well-organized state enterprises.[114] In such circumstances, it would clearly be possible for women to join the colony once a settlement had been established, and that is how I imagine it was done.[115] But our sources do not allow us to be definite about details of procedure. All I would claim is that, if we wish to find out the truth about the women, we only need to think about the gods.[116] ||

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[111] L. Deubner, "Ololyge und Verwandtes," *Abhand. Preuss. Akad. Phil. Hist. Klasse*, 1941 no. 1. The use of the term for the wailing of mourners, which Deubner thinks is post-homeric, is discussed by M. Alexiou, *The ritual lament in Greek tradition* (Cambridge, 1974; see especially 4-23).

[112] *Op. cit.* (n. 99 above), 56ff.

[113] E.g. the first settlement at Zancle; Thuc., vi.4.5.

[114] This is becoming clearer all the time as we learn about Greek colonies through archaeology; compare Megara Hyblaea, where there is a strong impression of organization from the beginning. See, e.g. G. Vallet, "Espace privé et espace public dans une cité coloniale d'occident, Mégara Hyblaea," *Problèmes de la terre en Grèce ancienne*, ed. M. I. Finley (École pratique des hautes études. Civilisations et sociétés 33, Paris 1973) pp. 83-94.

[115] Cfr. Plato, *Laws*, 925 b 4f., where the marriage between a colonist and a woman of the mother city is envisaged. On subsidiary immigration to Greek colonies, see A. J. Graham, *Colony and mother city in ancient Greece* (Manchester, 1971) pp. 64-66.

[116] To adapt Macan's description of Herodotus' historical principle, "cherchez la femme et n'oubliez pas le Dieu;" *CAH*, v.407.

18. THE *ODYSSEY*, HISTORY, AND WOMEN

If we include the goddesses and semidivine women, the *Odyssey* presents a great panorama of womanhood. This is a fascinating prospect for Greek historians, starved as they are for information about women in Greek history generally, and especially in the early period. Before they can use that material for historical reconstructions and conclusions, however, it is necessary to consider the question of the *Odyssey* as historical evidence. Because that question is not only complicated and multifaceted but also very important, it cannot be balked at or skimped, and it must be dealt with before the women of the *Odyssey* can be used for history.[1]

Very many and diverse theories have been put forward about the nature and composition of the *Odyssey*—theories that I, as an ancient historian, am not very well qualified to judge. However, I am glad to see that there is now a reasonable consensus in favor of the opinion, in which I have always believed, that the poem is a unified work of art, composed by a single poet at a single time.[2]

This enables the historian to treat the poem, even though it is primarily a literary work of fiction, as a piece of historical evidence. Modern historians, blessed—or even embarrassed—as they are by a superabundance of sources, would never think of using an epic poem as historical evidence, but it is a different matter for the ancient historian who is desperately short of source material, particularly in the early period, whenever exactly that may be, when the *Odyssey* was composed. This brings us to the all-important question, for a historian, of date. Can we establish when the *Odyssey* was composed?

It must be confessed straightaway that what we would most like to have—a precise and reliable external date—does not exist. The dates for Homer given by later Greeks were manifestly not based on definite knowledge. When Herodotos says (2.53.2) that he thinks

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[1] The very extensive bibliography on this subject can be reached by consulting recent works with very full lists, e.g. Morris, 1986, 130-38; Raaflaub, 1991, 252-56. I therefore confine my bibliographical references to either those strictly pertinent or representative selections.

[2] See, e.g., Heubeck, West, and Hainsworth, 1988, 6-7.

The Distaff Side: Representing the Female in Homer's Odyssey (1995), 3-16

that Hesiod and Homer lived four hundred years before his own time and not more, he reveals clearly that their date was a matter of uncertainty and dispute. His estimate puts the two early epic poets in the ninth century BC.

In default of reliable external dating, scholars have used a variety of inevitably less satisfactory methods to try to establish the date of composition of the Homeric epics: internal evidence of various kinds, linguistic development, literary comparisons and development, and evidence from archaeology and art.

The internal evidence consists of things mentioned in the poem, places, peoples, or material objects that it seems possible to date by other means. For example, the references to Sicily in the *Odyssey* are thought to provide us with a *terminus post quem* for its composition, namely, the reopening of the western Mediterranean to the Greeks about the middle of the eighth century BC. On this argument the *Odyssey* would have been composed after *ca.* 750 BC. A *terminus ante quem* has been seen in the introduction of the type of warfare based on heavily armed infantrymen, the so-called hoplites, and the associated armor and weapons, the earliest evidence for which is dated to the period *ca.* 700-675 BC. The 'orthodox' view has been that the fighting in Homer is very different from, and therefore earlier than, hoplite warfare.[3] Recently there has been a strong movement in Homeric scholarship in favor of the 'unorthodox' idea that Homer's fighting, too, was based on massed infantry.[4] If correct, that interpretation would weaken the chronological significance of the introduction of hoplite tactics. However, the arguments supporting the unorthodox interpretation can be shown to be unconvincing.[5] So it is still justifiable to argue that the introduction of hoplite tactics rep-

[3] Cf., e.g., Murray, 1980, 54 and 120.

[4] See, e.g., Raaflaub, 1991, 225-30; Wees, 1988 (from whom I borrow the terms 'orthodox' and 'unorthodox') 1-2.

[5] In establishing the unorthodox interpretation Latacz, 1977, has been very influential, but his arguments from Homer's words for military formations were already shown to be uncertain by Leimbach, 1980, in his review. The authority of Pritchett, 1985, 1-33, also added weight to the unorthodox interpretation, but his reliance on analogies often very distant in place and time (e.g., the Battle of Agincourt!) and on an inaccurate classification of Homer's many fights between individuals as formal *monomachiai* indicates the weakness of his case. The very full, detailed, and convincing analysis by Wees, 1988, may be seen, positively, as an excellent description of Homeric battle tactics and, negatively, as a complete rebuttal of the unorthodox interpretation.

resents a *terminus ante quem* for the composition of the Homeric poems. The references to Sicily and the introduction of hoplite warfare were two of the main arguments used by H. L. Lorimer in her impressive *Homer and the Monuments*, a very detailed and lengthy comparison of the objects in the Homeric poems and those in the archaeological record, based largely on the scholarship up to the Second World War.[6] From these and other indications she came to a time bracket for the poem's composition between *ca.* 750 and 675 BC.[7] ||

A more recent use of an internal dating argument has been based on the Homeric references to Egypt, especially to the great city of Egyptian Thebes (mentioned at *Iliad* 9.381ff.). W. Burkert has argued that this passage could not be a reminiscence of the great Bronze Age city, preserved in an old poetic formula by the bards who preceded the poet or poets of the *Iliad* and the *Odyssey*, because the language of those lines shows recent elements.[8] So he proposed to see the passage as a reference to the rebuilt Thebes, supposedly the capital of the Twenty-fifth Dynasty, which lasted from *ca.* 715-663 BC. He therefore argued that these dates were a *terminus post quem* for the composition of the *Iliad*, which he dated *ca.* 650 BC. The *Odyssey*, which is universally agreed to be, if not contemporary with, then later than the *Iliad*, would on this argument not have been composed earlier than about the middle of the seventh century. But Burkert's argument is quite unconvincing. The magnificence and wealth of Egypt and its greatest city in the Bronze Age were obviously proverbial, and these references in the Homeric poems offer no chronological precision. Nor is it at all probable that the activities of the short-lived Ethiopian Dynasty, which never established control over all of Egypt and whose capital was probably not at Thebes, would produced references to the greatness of Egypt and Egyptian Thebes in the Homeric poems.[9] Finally, the whole basis for Burkert's argument is removed by the observation that old

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[6] More recently, *Archaeologia Homerica*, founded by F. Matz and H.-G. Buchholz, 1967-, has been covering more comprehensively similar (but more extensive) ground to Lorimer's.

[7] Lorimer, 1950, 462-64.

[8] Burkert, 1976.

[9] For a full account of Egypt under the Twenty-fifth Dynasty, see James, 1991, esp. 677-708. I am grateful to David O'Connor for advice on this subject. On Homer and Egypt, see Braun, 1982, 32-35.

elements in the poems can be described in recent language.[10] This rather obviously failed attempt to use internal evidence for a chronological conclusion would not have deserved a mention, had the thesis not been adopted in S. West's part of the Introduction to the new *Commentary on the Odyssey*, with the result that a mid-seventh-century date for the composition of the *Odyssey* is likely to have wide dissemination.[11]

A chronological argument from literary development can be made on the basis of the reasonable assumption that Homer (and Hesiod) are earlier than the lyric poets.[12] Among the early lyric poets, Archilochos can be confidently dated about the middle of the seventh century,[13] so the Homeric poems would be earlier than that. The chronological conclusions are very imprecise, however, and not all experts in Greek literature would even accept the premise that the Homeric poems are necessarily earlier than the early lyric poets.[14]

A different argument from literary development was put forward by Morris, who believes on the basis of analogies from other cultures and times that the *Iliad* and *Odyssey*, being oral compositions, could not have been preserved until and unless they were written down. Otherwise, each new oral performance would have altered the poems. || This gives him a *terminus post quem* for their composition in the introduction of alphabetic writing to Greece, dated "close to *c.* 750 BC."[15] (He cannot find a *terminus ante quem* on any such arguments, so for that he accepts dates reached by other methods.) However, an argument based on comparative analogies does not constitute proof, and this one is by no means universally accepted.[16]

It is a relief to turn to the more objective combination of linguistic and literary criteria used by R. Janko in his important book, *Homer, Hesiod and the Hymns.*[17] His method was to study the variations between older and newer linguistic usages in the *Iliad*, the *Odyssey*, Hesiod's *Theogony* and *Works and Days*, and the Homeric *Hymns*. By this means he was able to put these works in a relative

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10 Kirk, 1968, 95; Janko, 1992, 8-9.
11 West, 1988, 33-34.
12 See, e.g., the chronology in Easterling and Knox, 1985.
13 Barron and Easterling, 1985a, 117; Graham, 1978, esp. 72-86.
14 E.g., those mentioned previously who would date the *Iliad* and *Odyssey* to *ca.* 650 or later.
15 Morris, 1986, 83-94.
16 See, e.g., Nagy, 1992, 33-36.
17 Janko, 1982.

chronological order, namely, to confine ourselves to the works of interest to us, *Iliad*, *Odyssey*, *Theogony*, and *Works and Days*. For establishing absolute chronological intervals and dates, Hesiod was very important to Janko. Because we know that the *Theogony* and the *Works and Days* were composed by one man, the differences that Janko found between the two works indicate an interval no longer than the term of a man's active life. The differences between the *Iliad* and the *Odyssey* are no greater than those between the *Theogony* and the *Works and Days*. So Janko argued that the *Odyssey* and the *Iliad* could also have been composed within the timespan of an active human life.

Finally, Janko used Hesiod to give absolute dates to the works he studied. There are historical reasons for dating Hesiod's *floruit* to about 700 BC.[18] With this precise date to work from, Janko's preferred range of dates for the composition of the *Iliad* was *ca.* 750 to 725 and for the *Odyssey ca.* 743 to 713.[19] This is a very bold and ambitious chronological argument, and it undeniably contains many uncertainties. But it also has many interlocking chronological indications and many additional arguments, to which a brief summary cannot do justice.

In the category of archaeology and art the most sensational contribution to the chronology of the Homeric poems comes undoubtedly from the so-called Nestor's Cup, discovered in a grave on Pithekoussai, the island in the Bay of Naples called Ischia today.[20] This Late Geometric cup bears an incised inscription that may be translated as follows: "Nestor's cup was fine to drink from. But whoever drinks from this cup will immediately be seized by the desire of fair-crowned Aphrodite." The cup itself and other objects in the same grave may be dated *ca.* 720 to 710. It has been generally recognized that the humorous graffito shows a familiarity with epic poetry, and the reference to the cup of Nestor is inevitably reminiscent of *Iliad* 11.632-37. It is possible to argue—and it has been

[18] See, e.g., Janko, 1982, 94-98, 228-31; Barron and Easterling, 1985b, 93.

[19] Janko, 1982, 230-31. In Janko, 1992, 18-19, he now dates the *Iliad* to before 750.

[20] Meiggs and Lewis, 1988, no. 1; Hansen, 1983, no. 454, unfortunately marred by multiple misprints of the date. (These are corrected in the *Addenda et Corringenda ad CEG 1*, Hansen, 1989, 304.) I do not follow the restoration in the first line that Hansen supports because I find his formal arguments lead to a less satisfactory sense. See, however, Meiggs and Lewis, 1988, no. 1, apparatus. Cf. also Graham, 1982, 99.

argued—that this evidence does not necessarily prove that our *Iliad* was known in Pithekoussai and therefore also in Euboia, whence the colonists originated, before *ca.* 720, because it could merely attest the diffusion of the general epic tradition about the heroes, which we know from the Homeric poems.[21] But this possible argument has a look of special pleading. We are not just dealing with a reference to Nestor; it is specifically to his cup. Surely the easiest and simplest conclusion is that our *Iliad* was well known in the Greek world generally before *ca.* 720.

Although not so striking and not so early, we also have important artistic evidence bearing on the date of the *Odyssey*. There are no fewer than three representations of the blinding of Polyphemos on Greek painted pottery of the second quarter of the seventh century.[22] Once again it is possible to argue that the artists knew the story but need not necessarily have known Homer's version of it in *Odyssey* Book 9.[23] The easiest conclusion, however, is surely that it was precisely this very striking part of our *Odyssey* that led to the popularity of the scene with Greek vase painters.

We have now briefly looked at four different ways of trying to determine the date of composition of the *Odyssey*. Internal indications suggested the period *ca.* 750-675; an argument from the character of oral poetry gave a suggested *terminus post quem* of *ca.* 750; a bold and ambitious use of linguistic and literary developments gave a date of *ca.* 743-713; and art and archaeology rather strikingly supported that dating because Nestor's Cup seems to show that the *Iliad* was composed no later than the third quarter of the eighth century, and the representations of the blinding of Polyphemos seem to show that the *Odyssey* was current before *ca.* 675, and at least that the low date for the *Odyssey*'s composition, *ca.* 650, cannot be right. Because these chronological arguments are independent of each other and yet they all point in general terms to the period *ca.* 750-700, there seems to be good reason for confidence that the composition

[21] See, e.g., Hansen, 1976, 42-43; Hiller, 1976, 30.

[22] See, e.g., Friis Johansen, 1967, 34-36; Fittschen, 1969, 192-94; Brillante, 1983, 103.

[23] Cf. e.g., Lowenstam, 1992, 166-68; Morris, 1986, 91-92; Kirk, 1968, 284-85. It is admittedly very possible to see representations of the Homeric epics in scenes that are too vague to bear the weight of the interpretation. This seems to me to be done, e.g., by Schefold, 1991, who dates Homer on these arguments to *ca.* 800; see 519.

of the *Odyssey* fell within this period. Naturally we cannot claim certainty—our evidence and arguments are not of that quality—but we have a probable hypothesis, which is the most we are likely to achieve in this early period of Greek history.

So the historian has in the *Odyssey* a piece of historical evidence with a fairly precise and secure date. There is still one issue to consider before we can proceed to use this evidence for historical purposes. It is widely and correctly believed that the bardic poetry of which the *Odyssey* is a part represented a very ancient tradition, || going back into the Bronze Age. Not only are there embedded in the Homeric epics descriptions of objects that did not exist after the Bronze Age but also the political geography can be shown to be that of the Bronze Age; the poet knows that he is describing heroic events earlier than his own time and employs some conscious anachronisms in order to do so. So the question inevitably arises, Is the *Odyssey* evidence for the world in which it was composed or for that earlier world—say, of the late Bronze Age—which the poet tried to depict? This was a matter of scholarly controversy in the past, but it is now generally recognized that the material that goes back into the Bronze Age is very restricted, and the general historical environment of the poem is that of the poet's own day.[24]

We have seen that that time is the second half of the eighth century BC. This period is characterized by historians of Ancient Greece as the beginning of the Greek Renaissance from the so-called Dark Ages, which followed the collapse of the great civilizations of the late Bronze Age. There are many aspects of this renaissance, such as economic and demographic growth and new achievements in art and architecture, but two of great importance were the growth in overseas trade and the beginning of the great movement of colonization of the Archaic period, which established new Greek communities over large parts of the coasts of the Mediterranean and Black Seas. The world of the *Odyssey* is preeminently one of seafaring, of new and strange lands reached by ship, and of the strange peoples who inhabited them. There is important evidence in the poem for the historian concerning both overseas trade and colonization, and some of that evidence relates to the role of women in both these activities. We may begin with trade.

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[24] Morris, 1986, 86-91.

One of the oddities in some modern works about early Greece has been the belief that normal, regular trade came relatively late. There seems to be a strange desire to interpret the undeniable exchange of goods well attested in the archaeological record as evidence, not of trade, but of gift exchange. It is certainly true that the *Odyssey* is full of examples of heroic gift exchange, but that should not blind us to the clear fact that it is also a storehouse of evidence about regular sea trade undertaken for profit.[25]

The regularity of the activity is revealed in the standard question asked of strangers who arrive by sea: "who are you, sirs? From what port have you sailed over the highways of the sea? Is yours a trading venture; or are you cruising the main on chance, like roving pirates, who risk their lives to ruin other people?"[26] (3.71-74). In many of the made-up narratives throughout the work, trading voyages occur, as ll those Odysseus himself is said to have undertaken in Book 19 (282-86). Such trading could be performed by the leaders of society. The fictional king Mentes, whom the goddess Athena pretends to be in Book 1, claims to be sailing to Temesa with a cargo of iron, which they intend to trade for copper (182-84); and Menelaos collects much livelihood and gold when sailing to Egypt (3.299-302, 311-12), Cyprus, Phoenicia, and Libya, among other destinations (4.81-91).

Special trading ships are mentioned, with the characteristic that they are broad (5.250; 9.323), and there are specialist traders, whose captain is mindful of his cargo and of the profits he has made (8.161-64). One particularly interesting feature is that we already meet the individual merchant who travels on another's ship (2.318-19; 14.295-96), a specialist in trading. We also have some indications of trade goods, though the *Iliad* offers a larger list, such as metals (1.182-84), papyrus (21.390-91), trinkets (15.415-16), and, notably, slaves.

Although this regular trading activity can perfectly well be carried on by Greeks, the *Odyssey* frequently mentions the foreign traders par excellence, the Phoenicians. They are providers of a fine bowl of silver and gold (4.615-19); in one of Odysseus' fictional tales a Phoenician trading ship sails from Crete to the western Peloponnese

[25] For a well-balanced and well-documented account, with full treatment of modern interpretations, see Wees, 1992, 219-48. Kopcke, 1990, 121-28, is also useful and well informed.

[26] The translations of this and all subsequent quotations from the *Odyssey* are by Rieu, 1946, and are published by kind permission.

and returns to Sidon (13.272-78); in another, Odysseus relates that a deceiving Phoenician takes him from Egypt to Phoenicia and later tries to take him on a trading voyage to Libya, with the secret intention of selling him as a slave (14.287-97). These stories suit well the other evidence we have for Phoenician activities in the eighth century and earlier.[27] We know of their settlements in North Africa, Spain, Sardinia and Sicily, and their trading efforts can be seen in archaeological finds in these and many other areas of the Mediterranean. These finds include fine metal bowls of silver and bronze.[28] Their connection with Crete in the *Odyssey* is especially interesting because there are striking Phoenician objects in the Cretan archaeological record[29] and a Phoenician shrine, attesting actual Phoenician presence, has recently been recognized at Kommos on the south coast of the island.[30]

The most memorable tale involving the Phoenicians is in the Eumaios story. When he was a boy, a Phoenician trading ship arrived in harbor with a cargo of trinkets. One of the slave women in Eumaios' father's house, Eumaios' nurse, was herself Phoenician, and she established close relations with the Phoenician sailors. So when they finally sailed after a whole year of trading and taking on cargo, the woman slipped away to sail with them, taking Eumaios with her to be sold as a slave (15.415-84).

This story finally brings us to women. The Phoenician slave || woman claimed that she had been captured by pirates and sold into slavery. There are several women slaves in the *Odyssey* who had been objects of trade. The most notable is Eurykleia, the old nurse, whose purchase by Laertes, Odysseus' father, for the price of twenty oxen is described in Book 1 (429-31). It is certainly true that the slave trade is prominent in the references to trade in the *Odyssey* because these transactions are a necessary device to explain the movements of individuals, but we need not doubt that the buying and selling of people as slaves was a regular feature of life at the time when the poem was composed, and that many of those slaves were women.[31]

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[27] Culican, 1991; Niemeyer, 1984.

[28] Culican, 1991, 478; Frankfort, 1954, 195-201; Markoe, 1985; note, however, the review by Winter, 1990.

[29] E.g., the inscribed bronze bowl from a tomb at Knossos; Coldstream, 1982, 271-72.

[30] Shaw, 1989.

[31] Wickert-Micknat has maintained that there are few slave women in the Homeric

Turning now to overseas colonization, two passages in the poem have been seen to reflect that experience.[32] The first is the description of the Utopian city of the Phaeacians, which is expressly stated to have been recently founded by an act of emigration and colonization:

> These Phaeacians had once lived in the broad lands of Hypereia, and had been neighbours to the Cyclopes, a quarrelsome people, who took advantage of their greater strength to plague them, till the day when their king, Nausithous, made them migrate and settled them in Scheria, far from the busy haunts of men. There he laid out the walls of a new city, built them houses, put up temples to the gods, and allotted the land for cultivation. (6.4-10)

The city's layout is carefully described at 6.262-72:

> Our city is surrounded by high battlements; it has an excellent harbour on each side and is approached by a narrow causeway, where the curved ships are drawn up to the road and each owner has his separate slip. Here is the people's meeting-place, built up on either side of the fine temple of Poseidon with blocks of quarried stone bedded deeply in the ground. It is here too that the sailors attend to the rigging of the black ships, to their cables and their sails, and the smoothing of their oars.

This might be a description of many Greek colonies where an isthmus site was chosen, such as Sinope in the Black Sea, which made the city easily defensible from the land, and provided the two harbors that were so useful. Later Odysseus is said to have "marvelled at the harbours with their well-found ships, at the meeting-place of these sea-lords and at their long and lofty walls, which were surmounted by palisades and presented a wonderful sight" (7.43-45).

Very different is the colonial passage from the story of the Kyklopes, where we are shown, not a well-established Greek colony, but a promising colonial site: ||

> Not very far from the harbour on their coast, and not so near either, there lies a luxuriant island, covered with woods, which is the home

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epics and that the situation about slavery and freedom was very different from what we know in other periods of the ancient world; see, briefly, Wickert-Micknat, 1982, 83, and, fully, Wickert-Micknat, 1983. But her attempt to deny the obvious rests on unconvincing a priori assumptions, improbable meanings attributed to Greek words, and weak arguments from silence.

[32] See, e.g., Beloch, 1924, 231-32; Schaefer, 1960, 77-78.

> of innumerable goats. The goats are wild, for man has made no pathways that might frighten them off, nor do hunters visit the island with their hounds to rough it in the forests and to range the mountaintops. Used neither for grazing nor for ploughing, it lies for ever unsown and untilled; and this land where no man goes makes a happy pasture for the bleating goats. I must explain that the Cyclopes have nothing like our ships with their crimson prows; nor have they any shipwrights to build merchantmen that could serve their needs by plying to foreign ports in the course of that foreign trade which ships have established between the nations. Such craftsmen would have turned the island into a fine colony for the Cyclopes. For it is by no means a poor country, but capable of yielding any crop in due season. Along the shore of the grey sea there are soft watermeadows where the vine would never wither; and there is plenty of land level enough for the plough, where they could count on cutting a deep crop at every harvest-time, for the soil below the surface is exceedingly rich. Also it has a safe harbour, in which there is no occasion to tie up at all. You need neither cast anchor nor make fast with hawsers: all your crew have to do is to beach their boat and wait till the spirit moves them and the right wind blows. Finally, at the head of the harbour there is a stream of fresh water, running out of a cave in a grove of poplar-trees. (9.116-41)

This may be seen as an idealized description of one of the favorite sites for Greek colonies, the offshore island.[33]

In the episode of the Kyklopes we are also vividly reminded of the fears of barbarous non-Greek peoples, whom colonists might meet. Often in the poem Odysseus is eager to learn of the inhabitants of some strange land "whether they are cruel, and wild, and unjust, or whether they love strangers and fear the gods in their thoughts" (9.171-76). In the case of the Kyklopes, they meet the worst examples of the first sort, murderers and cannibals.

It is in the relations of Greek colonists and native peoples that the women engage our attention. There is a theory, widely held today, that Greek women did not participate in the foundations of Greek colonies in the Archaic period and that only men went, who ensured the continuance of their settlements by taking wives from the local native population.[34] I have argued against this theory in print,[35] but it is hard to overturn a widely held belief.[36] So I make

[33] Cf. Beloch, 1924, 231-32; Bremmer, 1986.
[34] See, e.g., Rougé, 1970; Compernolle, 1983; Coldstream, 1993.
[35] Graham, 1984.
[36] Cf., e.g., Dalby, 1992, 19-20 n. 29, who shows no proper understanding of

no apology for returning to the subject now, especially as I think that the *Odyssey* can supply an additional argument to those I marshaled earlier. ||

One reason for the adoption of the theory against which I am arguing is that there is quite a lot of evidence for intermarriage between Greeks and non-Greeks, both within the colonial world and in general. But those instances of intermarriage are quite different from the belief that all the women in a new Greek colony were non-Greeks.[37]

A second support for the theory is a famous passage of Herodotos about the foundation of Miletos (1.146.2-3), which I quote:

> Those who set out from the prytaneum of the Athenians, and who consider themselves the most noble of the Ionians, did not take women to the colony, but married Carian women, whose fathers they killed. Because of this killing the women themselves made a law and imposed an oath on themselves (which they handed down to their daughters), never to eat with their husbands, nor to call their own husband by his name, for the reason that the men had killed their fathers, husbands and sons, and then, after doing that, married them.

This passage forms part of a patently tendentious chapter in which Herodotos is claiming that the Ionians are thoroughly mixed racially. The story also belongs to a well-known category, called *aetiological*; that is, it explains the reason for some custom or institution. The story's aetiological character is clearly shown by the information that the law, which the women had imposed on themselves, was passed on to their daughters, for this provided the necessary link between the existing customs and the events of long ago that were thought to explain them. An aetiological story cannot be taken as historical evidence for the facts it contains, but it could still be argued that it provides a general testimony to ancient Greek practices. This raises question whether Herodotos thought the supposed actions of Ionian settlers normal or abnormal. It seems to me that his words "did not take women to the colony" imply clearly enough that in Herodotos' opinion it was normal for colonists to include women. Even if this interpretation is not accepted, the Herodotean story about Miletos

the poverty of our source material and rejects my thesis without specific criticisms, except the phrase "a paper of patchy logic," for which an author would welcome precise justification.

[37] Graham, 1984, 293-94.

cannot be called sufficient evidence for the belief that in Greek colonization of the Archaic period it was the rule for the colonists to find their wives among the native population.

The remaining arguments in favor of the thesis are more or less uncertain instances of intermarriage in Greek colonization and the absence of mention of Greek women going as colonists. The latter, as an argument from silence, is inevitably suspect, and the more so in this case, because our Greek historical sources make a practice of || ignoring women. We have several reports of colonization in the fifth century by the best of historians, Thucydides. He invariably uses the masculine gender in his description of the colonists.[38] Yet we definitely know that the colonists who went to the Athenian colony of Thourioi in this very period brought their wives with them.[39] It is also striking that one Greek historian, writing, admittedly, about the Hellenistic period, regarded the presence of women and children as characteristic of a colonial expedition and their absence as indicative of a military campaign.[40]

Curiously enough, among the very few individual colonists whose names we know from the Archaic period, two are women.[41] Both these women were priestesses. This is the clue, I maintain, that leads to a very strong indirect argument against the theory that Greek women did not go to Archaic Greek colonies. That argument in brief, is the importance of Greek women in Greek religion.

It is easy to show, as has been stated, that "religion in Greece was suffused with women from top to bottom."[42] At the top were the great priestesses, very distinguished and powerful women, of the greatest importance to the community. There were frequently strict rules about eligibility for these high offices. But there were also many cults in which the worshippers were women, and any Greek woman had an important role to play in the religious life of the community.[43]

It is also an easy task to show that religion was of the highest importance in Greek colonies, just as in the cities of Old Greece.[44]

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[38] E.g. 1.27.1 (Epidamnos); 1.100.3, 4.102.2-3 (Amphipolis); 3.92.5 (Herakleia in Trakhis).

[39] Diodorus Siculus 12.10-11, esp. 11.1. Cf. Graham, 1984, 302.

[40] Diodorus Siculus 20.41.1. Cf. Meister, 1984, 396-97.

[41] Pausanias 10.28.3; Strabo 4.1.4 (C 179). Cf. Graham, 1984, 302-4.

[42] Schaps, 1979, 73.

[43] Cf. Graham, 1984, 304-10.

[44] *Ibid.*, 311-12.

I therefore argued that all Greek colonies would need Greek women in order to ensure a proper relation between the community and its gods, and it is absurd to imagine that native women, who did not even speak Greek, could be entrusted with these important tasks.

When I published this argument, I naturally drew attention to the passage in the *Iliad* where Theano, wife of Antenor, who has been chosen priestess of Athena by the people, is alone empowered to open the doors of the temple so that the goddess can be supplicated (6.296-300).[45] There is also archaeological evidence for great priestesses as early as *ca.* 800, if the occupants of the two very rich female graves discovered at Eleusis of that date were, as has been suggested, priestesses.[46] One of the graves contained a figurine of the Egyptian goddess Isis, whom the Greeks identified with Demeter.

It now seems to me that the *Odyssey* also can contribute to this question. In the great panorama of womanhood presented by the *Odyssey*, the non-Greek female element consists either of slave women or of divine or semidivine creatures, such as Kirke, Kalypso, the Sirens, Skylla, and Charybdis. While some of these characters had || definite attractions, they were all, if in different degrees, sinister and destructive.

Contrast these with the Greek women, either wives or maidens before marriage. Penelope obviously stands alone, but at the ideal Greek colony of the Phaeacians we find the distinguished queen Arete, whom the castaway Odysseus is advised to supplicate, and the ideal of an unmarried maiden, Nausikaa. These are the women that Greeks of the eighth century thought of as appropriate wives and partners in a Greek community. I would be the first to admit that this argument is impressionistic, but I would still maintain that the *Odyssey* here faithfully reflects a view of women that makes it impossible to imagine that Greek colonists would expect to establish a new Greek community in which the women were not Greek. ||

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[45] *Ibid.*, 304-6.

[46] Coldstream, 1977, 78-80.

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19. THASIAN CONTROVERSIES*

Introduction

My paper, 'The Foundation of Thasos',[1] evoked some sharp rejoinders from French experts in Thasian studies, which was not surprising, since it questioned some cherished beliefs. At the time I made no reply, because I felt I had made my case and had nothing to add. The issues of pre-Greek Thasos, the foundation date of the Greek colony, and its early days have, however, remained current, and I return to those matters in the first part of this paper.

In the second part I look again at the Great List of Theoroi. In this case I published a paper,[2] differing from some of the conclusions of Salviat,[3] a paper in which I offended against a fundamental principle of epigraphy, that one should not treat a text without autopsy of the inscription itself. My breach of that rule was a calculated risk, because I yield to no one in my respect for Salviat's work, and thought I could rely entirely on his account of the document.

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* I am very grateful to Harold Mattingly and Sara Owen, who kindly read parts of this paper in draft and made helpful suggestions, and to Alexandra Coucouzeli and Jorge Moreira for creating the figure. The following special abbreviations are used in this paper: FT = A. J. Graham, "The Foundation of Thasos," *BSA* 73 (1978) 62-98; 'Great List' = A. J. Graham, "On the great list of theori at Thasos," *Ancient World* 5 (1982) 103-21; *IG* = *IG* xii. 8; Martin, 'Colonie' = R. Martin, "Thasos colonie de Paros," *ASAA* 61 (1983) 171-7; Martin, 'Relations' = R. Martin, "Relations entre métropoles et colonies: aspects institutionnels," Φιλίας χάριν. *Miscellanea di studi classici in onore di Eugenio Manni* (Rome 1980) 4, 1435-45; Pouilloux, 'Fondation' = J. Pouilloux, "La fondation de Thasos: archéologie, littérature et critique historique," *Rayonnement grec: hommages à Charles Delvoye* (Brussels 1982) 91-101; 'Prosopographie' = "Prosopographie thasienne," *Rech.* ii (see below) pp. 253-311; *Rech.* i = J. Pouilloux, *Recherches sur l'histoire et les cultes de Thasos* i (*Études Thasiennes* iii, Paris 1954); *Rech.* ii = C. Dunant and J. Pouilloux, *Recherches sur l'histoire et les cultes de Thasos* ii (*Études Thasiennes* v, Paris 1957); Salviat, 'archontes' = F. Salviat, "Les archontes de Thasos," Πρακτικά του ή διεθνούς συνεδρίου ελληνικής και λατινικής επιγραφικής (Athens 1984) 233-58; Salviat, *BCH* 1983 = F. Salviat, "Catalogues des théores à Thasos: objections et réponses," *BCH* 107 (1983) 181-7; Salviat, *Thasiaca* = "Les colonnes initiales du catalogue des théores et les institutions thasiennes archaïques," *Thasiaca* (*BCH* Suppl. 5, 1979) 107-27.

[1] FT.

[2] 'Great List'.

[3] *Thasiaca.*

He did not, however, mention important facts, which he was able to use in his reply to my paper.[4] Since I have now had the opportunity to study the blocks of the inscription preserved in the Louvre[5] (and those in the museum at Thasos),[6] I can now return to this controversy.

Precolonial and Colonial

The main contentions of my paper on the foundation of Thasos were: 1) that the pottery found in the deep soundings of the Champ Héraklis and Champ Dimitriadis is truly precolonial, even though it was correctly dated by P. Bernard to, in general terms, the first half of the seventh century; 2) that the abundant Cycladic pottery, the earliest of which may be dated *c.* 650, thus provides the first archaeological evidence of the arrival of the Greek colonists from Paros; 3) that the literary evidence, widely thought to attest a higher date (*c.* 680) for the foundation of the colony, offers, when correctly interpreted, no better date than about the middle of the seventh century; 4) that we can, therefore, conclude that there is no conflict between the indications of the two categories of evidence, and the foundation of the Greek colony should be dated *c.* 650.

In addition, there were some subsidiary conclusions and speculative suggestions, viz.: 1) that the literary evidence does not show that Telesicles, the father of Archilochus, was the *oikistes* of the Greek colony; 2) that the colonization was probably forcible; 3) that the opportunity for Greek colonization in the Thracian region may have been offered by the ending of Phoenician interests in that part of the Mediterranean.

Apart from brief expressions of dissent, three substantial replies to my paper were published, two by Martin and one by Pouilloux.[7] It

[4] *BCH* 1983.

[5] It is a great pleasure to acknowledge my kind reception by the Director and Staff at the Louvre, who made the conditions for my work ideal. I also thank the Research Fondation of the University of Pennsylvania for a grant to finance my travel, both to Paris and Thasos.

[6] I am most grateful to Dr. H. Koukouli-Chrysanthaki, Ephor at Kavala, and to her Staff for allowing me to study the inscriptions in the Museum of Thasos, and providing excellent facilities. I was not able to see the block Thasos Inventory 249 (= *Rech.* i. 35), as it could not be located.

[7] Martin, 'Colonie' and 'Relations'; Pouilloux, 'Fondation'.

was naturally flattering that two such distinguished students of Greek Thasos should feel the need to take account of my work, but they did not read me very carefully. For example, both authors state that I accepted the idea that Tellis should be identified with Telesicles,[8] whereas I pointed out the difficulties that beset such a hypothesis and rejected it.[9] And Martin actually wrote that I accepted 680 as the foundation date of the Greek colony.[10]

On the archaeological evidence, Martin accepted that the pottery from Bernard's deep soundings was precolonial, and that the succession from Thracian to Greek occupations is shown by the appearance of Cycladic pottery.[11] So far, therefore, he agreed with me, but his chronology was different. In one place he wrote that the Cycladic pottery dates from the second quarter of the seventh century, and in abundance from *c.* 650;[12] in another that the break between the indigenous and the Greek material comes at the turn of the eighth and seventh centuries.[13] He thus fudged the archaeological dating, and put it too high, even though the correct dating is well established.

Pouilloux' opinion about the archaeological evidence is more complex, and perhaps a little obscure. He accepts that there is a clear distinction between the pottery of the earliest settlement (and its orientation) and that of after *c.* 650, but he carefully avoids calling the former precolonial, while emphasizing that the latter, the pottery from the Cyclades and Greece proper, is not only very abundant but also very luxurious, thus corresponding to a prosperous and well-established Greek city.[14] Thus Pouilloux, although he does not re-affirm his conviction that the earlier pottery attests a period of co-existence between the indigenous population and the Parian colonists,[15] leaves it, by implication, unaltered.

Pouilloux also emphasized that dating by pottery cannot be closer than about 25 years, and criticized me for dismissing the chrono-

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[8] Martin, 'Colonie', 172; Pouilloux, 'Fondation', 94 n. 22.

[9] FT 81.

[10] Martin, 'Relations', 1436.

[11] Martin, 'Colonie', 173-4.

[12] *Ibid.*

[13] Martin, 'Relations', 1436: "Le coupure se fait au tournant des VIIIe et VIIe siècles."

[14] Pouilloux, 'Fondation,' 96.

[15] See his "Archiloque et Thasos: histoire et poésie," *Fond. Hardt* 10 (1964) 1-27, at 14-16, 22-3.

logical importance of the oriental ivory lions found in the Artemision, which Salviat had dated to at the latest the second quarter of the seventh century.[16] My reason for doing so was not only the uncertainty of the dating of the ivories, but also the well-known principle that abundant painted pottery is more significant for chronology than a single luxurious object.[17] I maintain that point of view. Uncertainty about the chronology of the archaeological evidence remained a standard reaction for Pouilloux,[18] yet he oddly accused me of saying that these datings are subject to revisions and compromises, and of relegating the archaeological evidence to a subordinate position.[19] It was not I who revised the date of the early pottery, but Bernard. The compromises that I pointed to were not in the dating of the material, but in its historical interpretation.[20] Far from playing down the chronological importance of the archaeological evidence, I made plain that I regarded it as at least equal to that of the literary evidence.[21] We may now turn to that literary evidence.

The older French view was that the literary evidence shows us three phases in the Parian colonization of Thasos: 1) Tellis, Archilochus' grandfather, and Kleoboia bring the cult of Demeter to Thasos (Paus. 10.28.3); 2) Telesicles, son of Tellis and father of Archilochus, founds as *oikistes* the Parian colony of Thasos (Steph. Byz. s.v. Θάσος; Eusebius, *Praep. Evang.* 6.7.8); 3) Archilochus comes to Thasos as a reinforcement of the existing colony (Archilochus' poems and Parian inscriptions). Martin reiterates this reconstruction,[22] but Pouilloux had become more hesitant about the presumed first phase.[23]

My discussion of Pausanias' passage about Kleoboia and Tellis reached the conclusion that it is impossible to base upon it any historical role for Tellis in the foundation of Thasos.[24] Martin objected that my obscure discussions of Kleoboia's age are not sufficient to justify the rejection of Pausanias' account as fiction.[25] But it was

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[16] See Pouillox, 'Fondation', 96.
[17] FT 86-7 n. 249.
[18] Apart from Pouilloux, 'Fondation', 96, 99, cf. also his "Grecs et Thraces à Thasos et dans le Pérée," *Mélanges P. Lévêque* (*Annales Littéraires de l'Université de Besançon* 377, Paris 1989) 2, 367-73, at 368.
[19] Pouilloux, 'Fondation', 96.
[20] See FT 72, 86.
[21] See FT 86.
[22] Martin, 'Colonie', 173; Martin, 'Relations', 1437, 1441-2.
[23] Pouilloux, 'Fondation', 94-5 n. 22.
[24] FT 81-2.
[25] Martin, 'Relations', 1437 ('discussions confus').

Tellis' age that I discussed. If my discussion seemed obscure to Martin, I regret it; I tried to make complicated matters clear. Martin contended that 'the religious phase' is frequently found in Greek colonization,[26] but all that his examples showed was that the cults of the metropolis were established in the colony, not that those cults were established before the foundation of the colony. Pouilloux, on the other hand, admitted that the first stage, that of Kleoboia and Tellis, still remains enigmatic, and, though he continued to accept Pausanias' information, to explain it with certainty is a different matter.[27]

More substantial arguments concern the second stage, that of Telesicles. The sole evidence for this is the Delphic oracle preserved by Steph. Byz. and Eusebius (see above), which I quote without punctuation:

ἄγγειλον Παρίοις Τελεσίκλεες ὥς σε κελεύω
νήσῳ ἐν Ἠερίῃ κτίζειν εὐδείελον ἄστυ.

This oracle is generally recognized to be inauthentic, and was probably concocted as part of a biography of Archilochus composed at Delphi in the second half of the fifth century or first half of the fourth.[28] Both Martin and Pouilloux conceded that the oracle may be inauthentic,[29] but they argued that it could still represent a genuine tradition that Telesicles founded Thasos. It was necessary for them, therefore, to refute my detailed exegesis of the oracle.

What I showed was that, while it is possible, if we consider grammar alone, to understand the oracle to mean "Announce to the Parians, Telesicles, that I bid you found a conspicuous city in the island of Eëria,"[30] if we take into account both Homeric parallels and the comments of Oenomaeus of Gadara cited by Eusebius, we can see that the correct translation is, "Tell the Parians, Telesicles, as I order you, to found a conspicuous city in the island of Eëria," the phrase ὥς σε κελεύω being parenthetic. Understood in that way the oracle does not show that Telesicles was *oikistes* of Thasos, and

[26] Martin, 'Relations', 1441.
[27] Pouilloux, 'Fondation', 94-5 n. 22.
[28] See the discussion in FT 79, to which we may now add C. Marcaccini, "Tradizione biografica di Archiloco a Delfi," in *Appunti storici sul santuario delfico, Annali dell' Università di Ferrara*, (Nuova Serie) Sezione VI—Lettere, vol. viii, 1 (1995) 5-46.
[29] Martin, 'Relations', 1438; Pouilloux, 'Fondation', 97.
[30] Translation of H. W. Parke and D. E. M. Wormell, *The Delphic Oracle* (Oxford 1956) 1, p. 66.

Oenomaeus' comments show that Telesicles was not so regarded in Antiquity.[31]

Martin[32] argued against me: 1) that I went against the consensus; but I was not alone in doubting Telesicles' role as founder;[33] 2) that the name Telesicles appears in Thasian onomastics; and 3) that I used an ingenious punctuation[34] to reject the unanimous tradition of Antiquity. The second argument merely shows that Telesicles was a Thasian (and Parian) name. As for the third, I would certainly never rely on punctuation, which, being entirely editorial, reveals no more than the way the modern editor takes the Greek. As for the unanimous tradition of Antiquity, apart from the oracle there is no ancient evidence.[35]

Pouilloux' line was that, since there are two equally possible ways of understanding the Greek, what we have is an ambiguous Delphic oracle, one meaning of which would make Telesicles *oikistes* of Thasos.[36] He thus ignored my demonstration from parallels that there is only one correct way to understand the Greek, only one way that it would have been understood by ancient Greeks.

The idea that Telesicles was *oikistes* of Thasos was and is hard to kill,[37] but on what seems to me an unarguably correct interpretation of the literary evidence, there is no attestation of the second, Telesiclean, phase.

That there was an Archilochan phase, to be dated *c.* 650 from the *floruit* of the poet, is common ground between us, though Martin strangely accused me of removing all the reasons for that chronol-

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[31] See FT 75-7.

[32] Martin, 'Relations', 1438.

[33] See FT 82 n. 207.

[34] "une astuce de ponctuation."

[35] At Martin, 'Colonie', 172, he also stated that I dismissed Telesicles' role as founder on the pretext that a *heroon* has not been found in the Agora, but that is simply not the case. Having found that there is no evidence for Telesicles as *oikistes*, I said that we should no longer be surprised at our failure to find a *Telesikleion*; see FT 82. Pouilloux misrepresents me in the same way as Martin; see Pouilloux, 'Fondation', 97.

[36] Pouilloux, 'Fondation', 96-7.

[37] Even I. Malkin, though he accepts my interpretation, is unwilling to dismiss Telesicles from his founder's role; see *Religion and colonization in Ancient Greece* (Leiden 1987) 59. Recently J.-Y. Marc wrote that Telesicles was the principal *oikistes*; see "L'Agora de Thasos," in *L'Espace grec* (Paris 1996) 105-113, at 110. Who are the others? Tellis? Archilochus? Glaucus, son of Leptines?

ogy and hesitating about the date of Archilochus.[38] In fact, I gave the evidence for the date, and accepted Jacoby's *c.* 650.[39]

With regard to the notion that Archilochus might have been the *oikistes*, Pouilloux asked, "has anyone really said that?"[40] They had, and I gave the references.[41]

Both Martin and Pouilloux argued that the oracle given to Archilochus—'Αρχίλοχ' εἰς Θάσον ἐλθὲ καὶ οἴκει εὐκλέα νῆσον (Eusebius *Praep. Evang.* 5.31.1)—shows that Archilochus came as a reinforcement of an existing colony. Even though Pouilloux concedes that the oracle was probably not authentic,[42] he, like Martin, argues from its precise wording. οἴκει is contrasted with κτίζειν of the oracle to Telesicles, and taken to show that the colony already existed. In fact, Archilochus is told to inhabit the famous island, which obviously did exist, whether already colonized by the Parians or not. It is not legitimate to assume that Θάσον must refer to the Greek colony, rather than to the island, the only descriptive word in the oracle. The wording of this inauthentic oracle cannot, therefore, be taken to show that Archilochus came to a colony already founded, and whether he did in fact must remain uncertain.

Martin argued that all the fighting against Thracians in Archilochus' poems occurred on the mainland, and this represented a phase of expansion and conquest later than the foundation of the colony on Thasos.[43] He based that interpretation on the *mnema* of Glaucus. As this is not a tomb, he assumes that Glaucus died and was buried on the continent, and that is reflected in the very unusual stone, brought from the continent, of which the *mnema* is constructed.[44] This imaginative suggestion is too hypothetical to serve as a historical argument. The idea that the fighting against Thracians was all on the mainland is necessary to Pouilloux' reconstruction too, though not stated by him, for he regards the colonization of Thasos as peaceable. A major argument for that in Pouilloux' work is the appearance of Thracian names in Thasian onomastics, which he hypothesizes

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38 Martin, 'Colonie' 172.
39 FT 86 and n. 244.
40 Pouilloux, 'Fondation', 95.
41 FT 80-81.
42 Pouilloux, 'Fondation', 98.
43 Martin, 'Colonie', 173; 'Relations', 1439.
44 Martin, 'Colonie', 173; 'Relations', 1437 n. 4.

shows that there was intermarriage between the first colonists and the indigenous inhabitants.[45] In my paper I agreed that the few definite Thracian names probably reflected marriages between Thasians and Thracians, but objected that those marriages could have occurred long after the foundation of the colony.[46] Martin shared my opinion.[47] The only addition I would make on this topic is that we now know from G. Herman's important work that such foreign names can also bear witness to *xenia* relationships.[48] The Thracian names borne by Thasians are evidence, therefore, of relations between families of the Thasian elite and Thracians, but they tell us nothing about the foundation of the colony.

My contention that the colonization was probably forcible follows from my chronology. We know that the Parian colonists already had control of Aliki, and hence presumably of the whole island, by *c.* 650.[49] We also know that they were establishing settlements on the mainland at the same time.[50] If, as I believe, the Greek city of Thasos was also founded at the same time, the easiest assumption is that this large-scale and ambitious colonizing enterprise was achieved by force. It follows that the fighting with Thracians attested by Archilochus' poems could well belong to the initial Parian colonization of the area.

The foregoing discussion explains why I did not feel that Martin's and Pouilloux' criticisms of substance required me to abandon any of my positions. Both authors also criticized my methods. They accused me of a hypercritical attitude in general,[51] and, in particular, of treating the ancient literary sources hypercritically.[52] That I

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45 See *Rech.* I, pp. 311-13; *Mélanges Lévêque* 2, 368-9. Pouilloux' list of Thracian names in the former place was reduced by F. Chamoux, who had the advice of Masson; see "L'île de Thasos et son histoire," *REG* 72 (1959) 348-69, at 350-1. After Chamoux' deletions, we seem to have the following Thracian names attested at Thasos, which were borne by members of the elite; Βοέτας (*IG* ii^2 33.18); Δάτας (*Rech.* i, Catalogue I, column 4, line 48); Νέστις (*IG* 275.14, but there is uncertainty here; see *Rech.* i, pp. 311-12 and Chamoux, 351); 'Οργεύς (*Rech.* i. 28.11; cf. Hdt. 7.118); Πόρις (*Rech.* i, Catalogue I, column 5, line 23); Πύρις (*ibid.* line 7; column 6, line 26); Σθόρυς (*IG* ii^2 17; cf. *IG* ii^2 24).

46 FT 92-3.

47 Martin, 'Colonie', 175.

48 *Ritualised Friendship and the Greek City* (Cambridge 1987), 19-21.

49 For the date, see J. Servais, *Aliki* i (*Études Thasiennes* ix, 1980) 16.

50 See, e.g., Martin, 'Colonie' 174; FT 94-5.

51 Martin, 'Relations', 1436; Pouilloux, 'Fondation', 99.

52 Martin, 'Colonie', 172; Pouilloux, 'Fondation', 97 ("n'est-ce pas solliciter les textes à l'excès?").

read the sources critically, I am happy to accept; whether that was hypercriticism, I leave to other readers of my paper to judge.

A more serious objection, if just, is that it was to support a prior theory or theories—the Phoenician retreat or the principle that all Greek colonization in an indigenous environment was forcible—that I proposed my lower dating of the colonization of Thasos.[53] I have certainly never imagined that all Greek colonization was by force, and no one who read my paper objectively could believe that my new dating depended on a theory about the Phoenicians. I distinguished quite explicitly between the uncertain hypotheses in my paper and its most secure conclusions, the Phoenicians belonging to the former, and the dating, which was based on quite different arguments, to the latter.[54]

It was unfortunate, therefore, that Y. Grandjean, in a more measured statement about my paper, repeated the objection that my dating was chosen to support a prior theory, and described Pouilloux' paper as a refutation of mine.[55] For the reasons given above, I submit that the attempts by Martin and Pouilloux to refute me were unsuccessful. Grandjean's own conclusion was that the colony was established in the second quarter of the seventh century, and embarked on vigorous expansion from the middle of the century.[56] The foundation date is slightly lower than the old consensus, but in general the reconstruction remains the same. Grandjean's work does, however lead to a potentially more productive area of debate, the archaeological evidence.

The apsidal house at the bottom of Bernard's sounding in the Champ Hêraklis was judged by Grandjean to be indigenous and precolonial,[57] but he felt able to date some of the Greek colonial material to before the middle of the seventh century.[58] Of the material

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[53] Pouilloux, 'Fondation', 100, with n. 47.

[54] FT 97.

[55] *Recherches sur l'habitat thasien à l'époque grecque* (*Études Thasiennes* xii, Paris 1988) 465-6.

[56] See *ibid.* 468.

[57] See *ibid.* 436-41. I was mistaken, when I wrote that there was no evidence for mudbricks in the construction of the house (FT 65, 88), as Grandjean rightly pointed out, *ibid.* 437. Although mudbricks did not figure in P. Bernard's final description, "Céramiques de la première moitié du VIIe siècle à Thasos," *BCH* 88 (1964) 77-146, they are mentioned in *BCH* 85 (1961) 935.

[58] See Grandjean (n. 55) 466-7 n. 4.

he listed I have already discussed the oriental ivory lions,[59] and the material from the Artemision attributed to the first half of the century will be treated below. That leaves the two Protocorinthian aryballoi, which were found in a sondage in the sanctuary of Athena on the Acropolis. These were not described or dated in the original publication, though they were illustrated;[60] it was Bernard who dated them to the earliest times of the colony's existence, which he put in the first half of the seventh century.[61] We now have C. W. Neeft's comprehensive study of the Protocorinthian subgeometric aryballoi.[62] The RH aryballos illustrated in *BCH* 1959 (above, n. 60) is found in Neeft's List LXXX L5 (pp. 190-191). The dating is "middle to late ovoid period," i.e. 650-630.[63] Neeft also took account of the aryballoi found in the Artemision excavations and placed them in the lists he regards as appropriate.[64] There is no example assigned to a date before 650. Nor are there any aryballoi found at Thasos or Aliki dated by Neeft to before 650; all are placed in the second half of the seventh century.

A different line is pursued by H. Koukouli-Chrysanthaki.[65] She raises the date of stratum W in the sondage of the Champ Hêraklis—the only definite occupation stratum of the lowest four strata[66]—from the first half of the seventh century to the eighth. That provides the space to take stratum W as precolonial, while maintaining the traditional foundation date of the Parian colony as early in the seventh century, for which she finds archaeological confirmation in Protoattic and Protocycladic sherds from the Artemision. Her argument for raising the date of stratum W is flawed, because the two sherds on which it is primarily based did not come from stratum W. The rim fragment of a skyphos with pendent concentric semicircles actually came from stratum Y,[67] and the fragment of a handmade vase with 'channelled' decoration came from the sondage in the Champ Dimitriadis.[68] As for the Protoattic and Protocycladic sherds from

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59 Above, p. 367-8.
60 *BCH* (1959) 782 fig. 13.
61 Bernard (n. 57) 146.
62 *Protocorinthian Subgeometric Aryballoi* (Amsterdam 1987).
63 See Neeft's pp. 340, 379. This aryballos is not in his chronological list, p. 339.
64 See his p. 435, collections, under Thasos.
65 *Protoistoriki Thasos. Ta nekrotapheia tou oikismou Kastri* (Athens, 1992) 717-22.
66 See FT 63-5.
67 Bernard (n. 57) 140-2 no. 220.
68 *Ibid.* 111-12 no. 81.

the Artemision, in default of full publication and description the argument is hard to evaluate, but one sherd is attributed to the Ad group at Delos,[69] and Weill stated that that group could go down to the middle of the seventh century.[70] The French excavators of the Artemision did not date any of the Greek pottery they found earlier than that time.

The most extreme advocate of the theory that the archaeological evidence proves that the colony of Thasos was founded at the beginning of the seventh century was undoubtedly N. Weill.[71] On the basis of the finds from the Artemision, the author offered the following synthesis about the first century of the colony. The first known settlement and the first state of the Artemision sanctuary were contemporary. In the first half of the seventh century we see a Greek city, "doubtless in its infancy,"[72] but very normally constituted around its cults. There was no precolonial phase on the site of the city of Thasos. In this period the colonists made only coarse pottery of indigenous type; figurines and fine pottery were all imported, and there was nothing from the Cyclades. About the middle of the century the community at Thasos became more numerous and relatively prosperous, and began to manufacture figurines and fine pottery of island, orientalizing, type, as well as to import these products from the Cyclades and Greece.[73]

The finds from the Artemision, the main basis for this reconstruction, emerged in great quantity from a very disturbed stratigraphy. Nevertheless it is clear that most of these finds did represent a mass of votives from the sanctuary, spread over a large area, because the finds were in a general way homogeneous, and there were joining fragments found at a great distance from each other.[74] In these circumstances the chronology depends almost entirely on

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[69] Koukouli-Chrysanthaki (n. 65) 721 and n. 70.

[70] *La plastique archaïque de Thasos, i. Le haut archaïsme* (*Études Thasiennes* xi, Paris 1985) 76-7.

[71] In the work cited in previous note.

[72] "dans l'enfance sans doute"; *ibid.* 210.

[73] *Ibid.* 205-211. The same ideas were expressed in Weill's paper, "Remarques sur les origines de Thasos: πόλις, χῶρα, περαία," *Mnimi D. Lazaridi: polis kai chora stin archaia Makedonia kai Thraki* (Salonica 1990) 491-2, of which we have unfortunately, only a brief summary, owing to the death of the author.

[74] See Weill (n. 70) 7, 12, and the brief description of the site and its excavation by Grandjean (n. 55) 312-13.

the stylistic dating of the objects themselves.[75] Some criticisms were made of Weill's identifications,[76] but for the period after *c.* 650, when the figurines are supported by very abundant painted pottery, there is no problem about the use of the sanctuary and the identity of its worshippers. Attention is, therefore, concentrated on the first half of the seventh century, when Weill thought that the sanctuary was already in use.

The stratigraphy was, for once, helpful for the early period. A fill was discovered at a deep level, which contained exclusively early material. This material showed that the date of deposition was early in the sixth century, and the fill was thereafter not disturbed.[77] From this early fill, called the Remblai Ancien, came all the material attributed by Weill to the first half of the seventh century. However, since the chronology provided by the Remblai Ancien, though good, is not close, more precise dating even of the material within it depends on stylistic criteria.

Two fragmentary figurines, nos. 97 and 98 (pp. 87-90; plate 23) are thought to have been made on Samos and to date from the first half of the seventh century.[78] The Samian origin of 97 is regarded as possible by Weill, but the typological parallels are not exact, so the supposed date of the supposed Samian parallels does not seem to me a clear or firm guide to the date of 97. Weill regarded 98 as certainly Samian, and here she has stronger indications. She judged it to represent a later stage of similar Samian figurines, which were dated to the beginning of the seventh century.[79] But the dating seems to me quite arbitrary, so any conclusion drawn from it about the date of the Thasian figurine seems insecure.

Numbers 69-72 (pp. 55-7; plates 12 and 13) are parts of pottery birds. Although these fragmentary birds do not belong to established series, and cannot even be fully reconstructed, their technical characteristics are sufficiently clear to indicate the ceramic groups to which they belong, namely, the so-called G 2-3 ware, of both the

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[75] See Weill (n. 70) 14-16, where it is also explained why stylistic criteria cannot provide a closely calibrated chronology.

[76] See the review by A. Moustaka, *Gnomon* 61 (1989) 231-5.

[77] See Weill (n. 70) 7, 13.

[78] The early date is also affirmed, *ibid.* 206, and was accepted by C. Rolley in his review of Weill's book, *RA* 1988, 130-2.

[79] See K. Vierneisel, "Neue Tonfiguren aus dem Heraion von Samos," *AM* 76 (1961) 25-59, at 36.

Aeolian and Lemnian categories.[80] 69 and 70 were classified as Aeolian and compared with an askos with a ram's head, which was also found in the Remblai Ancien.[81] Since Aeolian G 2-3 ware has been dated, in general terms, to the first half of the seventh century, Weill dated her 69 and 70 to the first half of the seventh century. Lemnian G 2-3 ware, to which 71 and 72 belong, on the other hand, continues into the second half of the seventh century, so those objects could be later than *c.* 650.

The dating of G 2-3 ware cannot be made too precise,[82] and Weill was tendencious, when she stated that G 2-3 ware corresponds to the very beginning of the century,[83] but, even if her dates are accepted, two pottery birds and one askos seem to be the only objects identified as belonging to the first half of the seventh century, a period of fifty years, during which Weill maintained the sanctuary was in use. It is no surprise that Grandjean wrote that one cannot fail to be struck by the small number of objects from the first half of the seventh century.[84] The contrast with the great mass of figurines and fine pottery of island, orientalizing, type, both imported and manufactured at Thasos, which begins *c.* 650, could not be greater.

It is this same small group of G 2-3 pots[85] that has to bear the whole weight of Weill's argument that the beginning of the use of the Artemision is contemporary with the habitations revealed in the deep soundings in Champ Héraklis and Champ Dimitriadis. Grandjean pointed to the disparity between the small group of G 2-3 pots from the Artemision and the abundant quantity of G 2-3 ware recovered from those soundings, not to mention the larger quantity that must be assumed to exist in the wider area of that early settlement.[86] The evidence is thus strongly against the idea of contemporaneity.

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[80] Weill (n. 70) 56-7. For G 2-3 ware, see Bernard (n. 57) 88-109; for the distinction between Aeolian and Lemnian, 105-106.

[81] See Weill (n. 70) 56 and fig. 15. The askos is described as certainly belonging to G 2-3 ware by Bernard (n. 57) 91 n. 1.

[82] See the discussions in Bernard, *ibid.* 92-3; FT 67-9; L. Beschi, "Materiali subgeometrici e arcaici del Nord-Egeo: esportazioni di Lemno," *Quaderni del CNR* 112 (1985) 51-64, at 52.

[83] Weill (n. 70) 206.

[84] Grandjean (n. 55) 439.

[85] *Ibid.* ('ce modeste lot').

[86] *Ibid.*

In general, what are we to think of a reconstruction that requires us to believe that a Greek colony should, for the first half century of its existence, neither possess the material objects used in its mother city, nor manufacture pottery of Greek type itself? That, after fifty years of such an existence, that Greek colony should suddenly receive not only abundant imports from the mother city, but also the artisans capable of making objects of the same type? Rolley not only accepted Weill's reconstruction, but claimed that it was supported by similar developments in other Greek colonies, where it was only after about a generation that colonies reached a second stage, a definitive and stable installation.[87] But the reality is quite the opposite. In Sicily, for example, the arrival of the Greek colonists at the time indicated by the literary foundation dates is unambiguously recognizable in the material record, and these colonists are immediately making pottery, including fine painted pottery, of Greek type.[88] The attempt to attribute Bernard's early settlement to an early colonial phase requires a reconstruction of the colonization of Thasos, which would be unique in all that we know of Greek colonization, a reconstruction much too improbable to be accepted.

The difficulties in the received interpretation of the French Thasian scholars are well illustrated by the variety of descriptive terms used by them for the early pottery of the types published by Bernard. Such pottery has been discovered in several deep soundings in, especially, the sector of the Dionysion and Artemision. This pottery is sometimes called précoloniales,[89] sometimes 'précoloniales'/'précolonial',[90] at other times céramique indigène,[91] céramique locale,[92] matériel de fabrication locale.[93] My favourite word is 'paracolonial',[94] which describes perfectly material neither precolonial nor colonial, material that both is and is not colonial.

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[87] Rolley (n. 78) 132.

[88] See *CAH* iii^2, 3, 103-9.

[89] *BCH* 104 (1980) 729 and 105 (1981) 935 (J.-J. Maffre and F. Salviat). The material was found in the Artemision in nearly two metres of layers of sand, lying on virgin soil and some natural rock.

[90] Weill (n. 70) 210 n. 3 (of the same material); *BCH* 107 (1983) 869 (A. Jacquemin).

[91] Grandjean (n. 55) 314 (of the material in *BCH* 107 (above)).

[92] *BCH* 109 (1985) 874 (Fr. Blondé, A. Muller, D. Mulliez).

[93] Grandjean (n. 55) 321 (of the material in the previous note).

[94] R. A. Tomlinson, "Archaeology in Greece," *AR for 1994-1995*, 50, reproducing the report by Fr. Blondé, A. Muller and D. Mulliez of their 1993 season's work in the Terrain Valma. They did not use this word, however, in their report of the same work in *BCH* 119 (1995) 681-2.

It seems to me, therefore, that neither the published replies nor the archaeological work of the last twenty years have shaken the 'working hypothesis' I put forward in 1978.[95] There are, however, two possible additional testimonies to the precolonial, Thracian, settlement at the site of the ancient city of Thasos, which I was unaware of, when I wrote my paper.

The first has been contributed by S. Owen's recognition that the Cave of Pan on the Acropolis was originally a Thracian rock-cut tomb.[96] As she rightly points out, this is not only the earliest, but also the only Thracian, tomb known at the city of Thasos.

The second place which may preserve indications of precolonial, Thracian, settlement is the Herakleion. It is widely believed, on the basis of literary evidence and some characteristics of the cult, that the cult of Heracles at Thasos existed there before the arrival of the Greek colonists.[97] All the buildings of which remains have been found at the Herakleion are, however, clearly of the colonial period.[98] The centre of the sanctuary, on the other hand, was the rock altar,[99] and that has been considered by some to have originally been indigenous.[100]

The rock altar was discovered by M. Launey, and is well described and illustrated in his book.[101] A rock altar as such can perfectly well be Greek, since an outcropping of bedrock was often used for an altar in Greek sanctuaries.[102] Greeks coming from Paros, in

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[95] FT 98.

[96] "New Light on Thracian Thasos: A Reinterpretation of the 'cave of Pan,'" *JHS* 120 (2000) 139-43.

[97] See FT 89-91; cf. also J. des Courtils and A. Pariente, "Problèmes topographiques et religieux à l'Hérakleion de Thasos," *L'espace sacrificiel*, ed. Roland Étienne and M.-T. Dinahet (Paris 1991) 67-73; B. Bergquist, *Herakles on Thasos* (Uppsala 1973) 35-9.

[98] The stages in the history of the sanctuary, and the functions of its buildings were in general convincingly and correctly analysed by Bergquist, *ibid.* 39-57. To the evidence she considered we now have to add the new discoveries reported in *BCH* 109 (1985) 881-4; 110 (1986) 802-6.

[99] See Bergquist (n. 97) 20: "the altar was the one indispensable feature of the sanctuary." Thus in her first stage there was a simple altar sanctuary (39).

[100] See, for example, the remarks of E. Will, N. Najdenova and des Courtils in *L'espace sacrificiel* (n. 97) 311-13.

[101] *Le sanctuaire et le culte d'Héraklès à Thasos* (*Études Thasiennes* i, Paris 1944) 27-30, plates III. 2 (photograph) and XVIII (plan). For the discovery of the rock altar ("une sorte de massif isolé taillé dans le roc"), see *BCH* 59 (1935) 292-3.

[102] See D. W. Rupp, "Reflections on the Development of Altars in the Eighth Century BC," *The Greek Renaissance of the Eighth Century BC*, ed. R. Hägg (Stockholm 1993) 101-107.

particular, would have been familiar with rock altars, for there was one in the Delion and another in the sanctuary of Aphrodite.[103] It is not a necessary hypothesis, therefore, that the rock altar of the Herakleion was necessarily indigenous. One feature of that rock altar, however, requires more extended discussion, the holes or pits in the rock called by Launey 'cupules'.[104]

Ten cupules, divided into two groups of five, are placed in two roughly parallel lines. The distance between the lines is from 0.70 to 1.00 m. Within that space are two smaller cupules. The shape and the sizes of the cupules vary considerably. Some are roughly circular; the largest of these has a diameter of 1.00 m. Others are roughly rectangular; the largest of those are 1.10 by 0.70 m. The depths of the main ten vary from 0.80 to 0.40 m.; the two small, intermediate, ones are 0.20 and 0.15 m. deep. In spite of these variations, Launey judged that they were all man-made.

One of the cupules was found full of ash, which was carefully sieved. It contained very small pottery sherds, none apparently very distinctive, and a few small pieces of bone impossible to identify.

Launey regarded the cupules as outside the altar.[105] The altar proper ends on its E. side in a roughly straight cutting, which produces the effect of a low wall. Beyond that the slightly lower rock contains the cupules.[106]

There have been various suggestions about the function of the cupules. Launey originally suggested that offerings were perhaps fixed in the cupules. That idea was perhaps prompted by the block of marble pierced vertically by a roughly square hole, which was found

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[103] The rock altar in the Delion is fully described and well illustrated by O. Rubensohn, *Das Delion von Paros* (Wiesbaden 1962) 5-7 and Beilagen 3a, 3b, 4a, 4b. Cf. also M. Schuller, *Das Artemistempel im Delion auf Paros* (Berlin, New York 1991) 2. Rubensohn's belief that the altar was already used for cult in the early Bronze Age (*ibid.* and 83) was questioned by N. M. Kontoleon, *Gnomon* 38 (1966) 203-4, but is, in any case, not a necessary part of the present argument. For the rock altar in the sanctuary of Aphrodite, see Rubensohn, *RE* s.v. Paros, 1846-7.

[104] Launey (n. 101) 28, with n. 2. In this paper I retain Launey's term 'cupules'. It should be noted, however, that in Bulgarian Archaeology this term is used for smaller holes in the rock; holes as large as Launey's cupules are called basins.

[105] *Ibid.* ("à l'Est du massif rupestre;" "cupules voisines de l'autel").

[106] This is seen clearly in Launey's plans, plates XVIII and XIX, and in the photograph, 28 fig. 10. Des Courtils and Pariente (n. 97) 67, also say that the cupules are to the E. of the altar.

in one of the cupules.[107] In his book, however, the marble block is not mentioned, and it was removed from the cupule before the photograph reproduced there was taken.[108] It seems that he must have decided that the marble block was in the cupule by chance or in secondary use. Whatever his reason, he abandoned his original suggestion, and left as the only clue to his interpretation his description of the contents of one of the cupules.

Bergquist originally suggested that the cupules were sacrificial pits (βόθροι), in which liquid or solid offerings were poured or placed. She thus envisaged an ensemble with two rites, burnt, flesh, sacrifices on the top of the rock (the βωμός), and the pouring or deposition of offerings in the βόθροι.[109] Pouilloux doubted that the cupules were used for offerings.[110] In a later treatment Bergquist thought that the cupules were either βόθροι or postholes, the latter interpretation having been suggested to her by Homer Thompson (by letter), who thought of a canopy over the altar, such as had been suggested for altars at Perachora and Halieis.[111]

The fullest treatment of canopies over or around altars known to me is that by D. W. Rupp in his Bryn Mawr dissertation of 1974, which was apparently not published.[112] Various terms have been used for such structures, such as 'baldaquin'/'baldacchino',[113] but Rupp prefers 'tetrastylon'. On the basis of representations on vases, and a small number of altars surrounded, or partly surrounded, by four bases for columns, Rupp defined tetrastylon as a "four-columned free-standing structure with a rectangular plan." The regular and formal architecture of the tetrastylon precludes any connection between such structures and the cupules.

The idea that the cupules were postholes was strongly advocated by des Courtils and Pariente.[114] They rediscovered the displaced

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[107] See *BCH* 59 (1935) 292 fig. 10.

[108] Launey (n. 101) 28 fig. 10.

[109] Bergquist (n. 97) 39.

[110] "Héraclès thasien," *REA* 76 (1974) 305-316, at 312.

[111] See Bergquist, "The Archaic Temenos in Western Greece. A Survey and two Inquiries," *Le sanctuaire grec*, ed. A. Schachter, *Fond. Hardt* 37 (1990) 109-158, at 141-2.

[112] "The Greek Altars of the Northeastern Peloponnese, *ca.* 750/725-*ca.* 300/275 BC", 359-75.

[113] See R. Ginouvès, *Dictionnaire méthodique de l'architecture grecque et romaine* iii (Rome 1998) 51.

[114] (n. 97) 68-9. In its essentials, the idea already appeared in *BCH* 110 (1986) 806.

marble block pierced by a roughly square hole, which was lodged in one of the cupules when found by Launey, and decided that all the larger cupules once held similar blocks, in which wooden posts were fitted. That interpretation was accepted by some of those attending the conference where it was presented, and suggestions were put forward about the nature and purpose of the structure postulated.[115]

E. Will thought that the presumed wooden barriers might be paralleled by what was found to the E. of the sanctuary of Zeus Polieus on the Athenian Acropolis, namely, large numbers of postholes, which were taken as evidence for a wooden barn and a fence, within which sacred oxen were housed before the *Bouphonia.*[116] Apart from the rather far-fetched parallel, the neat, regular and carefully aligned postholes on the Acropolis are not at all comparable to the cupules. S. Stucchi thought that a wooden fence limited a reserved area, an *abaton*, but offered no parallel. Des Courtils and Pariente, in spite of their confidence that the cupules were postholes, confessed that they did not have a satisfactory interpretation to propose for the wooden structure they were led to restore. The matter is further complicated by other holes, taken to be postholes, which have been discovered at the Herakleion.

In their excavations in the Herakleion of 1984 and 1985, des Courtils and Pariente discovered an earlier building (called by them Temple A) under the peripteral temple (Temple B).[117] Close outside and within Temple A they found, in their first season, holes in the rock, and even in the sand, which they took to be postholes associated with the structure of the building.[118] In their second season, however, they found more 'postholes', whose positions were not easy to reconcile with their previously suggested architectural reconstruction.[119] Furthermore, in their investigation of the foundations of Temple B, they found more 'postholes', at least 11 by my count, irregular both in form and disposition.[120] This group is crudely in

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[115] See the discussion in *L'espace sacrificiel* (n. 97) 311-13.

[116] See G. P. Stevens, *The Setting of the Periclean Parthenon* (*Hesperia* Suppl. 3, 1940) 79-86.

[117] See *BCH* 109 (1985) 881-4; 110 (1986) 802-6, with an informative plan, fig. 16. Unfortunately, no clear or definite evidence was discovered, by which to date Temple A; see the discussion at *BCH* 109, 884.

[118] See *BCH* 109, 802 and figs. 17, 18.

[119] See *BCH* 110, 802 figs. 17, 20.

[120] *Ibid.* 805 and fig. 23.

line, and the holes are very close together, some overlapping. Although none of the 'postholes' discovered by des Courtils and Pariente are as large as the main cupules, they are quite big, and could have received a substantial timber. It is obviously uncertain whether one should postulate any connection between these newly-found holes and the cupules, but it is difficult to divorce them from the discussion. One thing seems clear: it is very hard to associate these 'postholes' with any structure known in Greek architecture.

Among those who contributed to the discussion mentioned above,[121] Najdenova stood apart. She said that, in order to understand the cupules, it was necessary to take account of archaeological evidence from rock sanctuaries in Thrace. These sanctuaries are characterized by larger or smaller basins cut in the rock, linked by troughs. The features briefly mentioned are more fully described, though unfortunately not illustrated, in her paper, "Rock Shrines in Thrace."[122] To judge by the description, these features are not exactly parallel to the cupules, but there are rocks with rock-cut basins in Thrace, which appear strikingly similar to the rock altar of the Herakleion and the cupules.[123] In any case, if Najdenova, who knows the Thracian material, regarded the features found in Thrace as comparable, her opinion must carry weight, and she thought that the cupules were made to receive offerings.

If the holes in the rock at the Herakleion are taken to be postholes, there are also Thracian parallels, and from an ideal geographical location, the settlement site at Kastri in the S. of the island of Thasos.[124] Posthole buildings were discovered here, of which the most significant was a large apsidal building, the only one investigated (though not completely). The evidence found for dating is not fully satisfactory, but the posthole buildings belong in general to the Late Bronze Age. The postholes at Kastri are numerous, often set close together, vary in shape and size, but are often quite large.[125] In all these characteristics, therefore, they are like the "postholes'

[121] See n. 115.

[122] *Studies in Settlement Life in Ancient Thrace*, ed. V. Velkov and P. Petrov (Sofia 1990) 85-100, at 88.

[123] See *Megalitite v Trakiya ii, Trakiski Pametnitsi 3* (Sofia 1982) plates 157-9. I am very grateful to Sara Owen for kindly drawing my attention to this.

[124] See H. Koukouli-Chrysanthaki (n. 65) 703-5; plate 362, α and β.

[125] The postholes are particularly well seen in *BCH* 110 (1986) 731 fig. 99.

found at the Herakleion. While it was possible at Kastri to see a ground plan of an intelligible building, that is not the case with the 'postholes' at the Herakleion, but it must be remembered that the rock at the Herakleion has been cut and levelled, perhaps many times.[126] It was not possible to make sense of the holes in the rock at the Herakleion in terms of Greek religion or of Greek architecture. They may always remain mysterious, but it seems to me most likely that they were made by Thracians.

In spite of our differing interpretations, there is one thing which is agreed by all those who have discussed the precolonial and earliest colonial evidence from Thasos: we need more evidence. Bernard stated that the questions raised by his deep soundings could only be resolved by less restricted excavations.[127] The need for further excavations was repeated by me[128] and by Grandjean.[129] Most recently, A. Muller has reiterated the need for a new, more extensive, excavation in the Champ Héraklis.[130] It is good to know, therefore, from the same source, that such an excavation will now take place, and we can look forward to progress on the important questions here discussed.

The Great List of Theoroi

The ancient Thasians inscribed long lists of their chief officials, the archons, who were eponymous and normally served in colleges of three, and the theoroi, who also served, for most of their history, in annual colleges of three. Parts of four such lists, two of the archons and two of the theoroi,[131] have come down to us. The most substantial remains are of the list of theoroi (hereinafter "the great list") which was inscribed on the inside of one, or possibly both, of the walls of the Passage of the Theoroi.[132] Those walls fell down or were

[126] Cf. e.g., Bergquist (n. 97) 19-20.
[127] Bernard (n. 57) 146.
[128] FT 98.
[129] Grandjean (n. 55) 319.
[130] In his paper, "L'archéologie de la fondation de Thasos: lecture critique de la stratigraphie du sondage 'Héraclis Kokinos,'" given at the conference *Thasos d'Archiloque aux guerres médiques*, held at Lille, March 25th 2000.
[131] I was mistaken when I stated that we know of three lists ('Great List', 103).
[132] It is thought that the great list was very largely, or totally, confined to the

dismantled long ago.[133] However, the many inscribed blocks that we possess, either physically or in copies, offer architectural and prosopographical clues to their original position, and generations of scholars have devoted their labours to trying to reconstruct the wall and rediscover the placement of the inscriptions on it. That work was greatly facilitated by the important early discovery that the appearances of fathers and sons, and other family relationships, show that each column contained the names of roughly one generation.

In his paper of 1979 in *Thasiaca*, Salviat made important new contributions to this endeavour. He argued that, at the bottom of the wall above the socle, there were four courses 0.465 m. high; above them four courses 0.385 m. high; then three courses 0.31 m. high; and finally, at the top, one course 0.295 m. high. The original inscribed list is found solely on blocks 0.385, 0.31, and 0.295 m. high,[134] and Salviat therefore argued that it occupied, in many columns, eight courses of the wall. The novelty of Salviat's approach lay not only in new methods of establishing the number of courses of the wall and of the courses originally inscribed, but also in the additions to the latter that he proposed. A third course of 0.31 blocks, and a fourth of 0.385 blocks, were radical new suggestions.

Nothing in the first five columns, which formed the main subject of Salviat's paper, requires a third course of 0.31 blocks, and these columns can be reconstructed on the assumption that there were only two courses of that height. I suggested, therefore, that one of Salviat's 0.31 courses could be dispensed with. In his reply Salviat pointed out that there is good evidence for three courses 0.31 m. high in column 9 (Fig. 4).[135] *IG* 277H is firmly placed there by its connection to the large joining group of blocks long recognized, where it is in the course of 0.31 blocks immediately above those of

W wall; see Salviat, *Thasiaca*, 111 and Fredrich, *IG*, p. 89. For the Passage, see my discussion in "Thasos: the Topography of the Ancient City," *BSA* 95 (2000) 301-27.

[133] Still *in situ* in the E wall are parts of the three lowest courses of the wall. See Salviat, *Thasiaca*, 109; G. Roux, "Le 'passage' des théores et le probléme du prytanée thasien," *BCH* 79 (1955) 353-64, at 358 fig. 22. These blocks are not inscribed.

[134] The first, recapitulatory, part of the great list, which stretched into column 7, gave the names of all the theoroi known before the time when the list was inscribed, which can be put in the first half of the fourth century; see *Rech.* i, p. 263. Thereafter the names of the theoroi who subsequently held office were added to the inscription down to the Roman imperial period. The inscriptions on blocks 0.465 m. high are all of a very late period; see Fredrich, *IG*, p. 89.

[135] *BCH* 1983, 181-4.

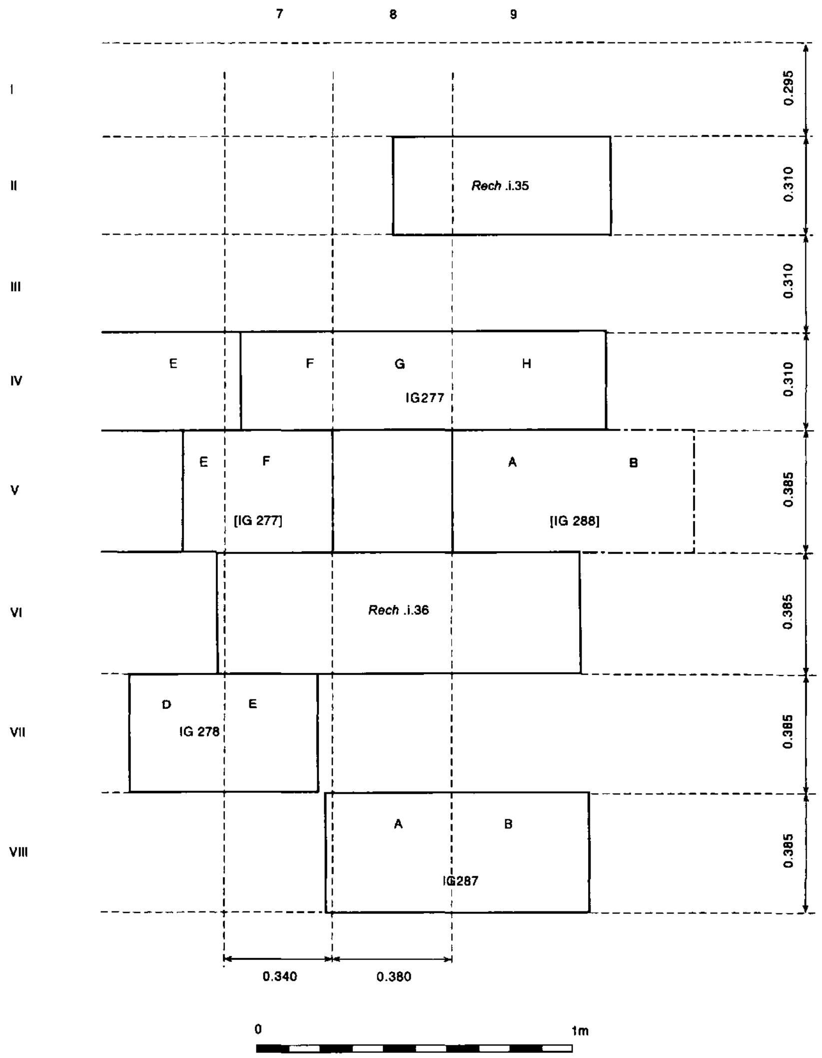

Fig. 14. Reconstruction of columns 7, 8, 9 of the great list of Theori according to Salviat

0.385.[136] *Rech.* i.35 is also 0.31 m. high, and its second column is shown to belong to column 9 by very strong prosopographical arguments.

No fewer than four of the theoroi in this short list appear in the second column of the archon list, *Rech* i.34, viz.:

> Τιμοκλῆς Χοίρου, line 3; *Rech.* i.34.41.
> Πύθιππος Παιεστράτου, line 6; *Rech.* i.34.46.
> Ν[α]υσικύδης Ἀριστοφῶντος, line 7; *Rech.* i.34.43.
> Ἀριστοφάνης Ἐρατοκράτευς, line 9; *Rech.* i.34.47.

In addition, the father of another of the theoroi appears in the first column of the same archon list: Ἀμφηρίδης Σιμαλίωνος, line 1; cf. Σιμαλίων Ἀμφηρίδευς, *Rech. i.* 34.13.[137]

Rech. i.34 is perfectly securely dated by multiple prosopographical connections.[138] Its first column was dated by Pouilloux 345-315; its second 315-285; its third 285-255. Thus the first column covers the same period as the second half of column 7 and the first half of column 8 of the great list, and the subsequent columns accordingly. The second column of *Rech.* i.34 is thus chronologically equivalent to the second half of column 8 and the first half of column 9 of the great list.[139] *Rech.* i.35, being 0.31 m. high, belonged to the first half of the columns of the great list. Its second column must, therefore, form part of column 9.

Yet *Rech.* i.35 cannot be placed immediately above *IG* 277H. There is a hole for the pinch-bar in the bedding surface of the block *IG 277* F-H, 0.28 m. from the RH edge of the block. This hole shows there was a joint in the course above it at this point. If *Rech.* i.35 were placed in the position required by the pinch-hole, the columns of names would not be in line from course to course. *Ergo*, a third course of 0.31 blocks is a necessary postulate. I have confirmed the existence and position of the hole for the pinch-bar by autopsy, and I regard Salviat's argument as completely convincing. Consequently, I withdraw my suggestion that there were only two courses 0.31 m. high.

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[136] See *Thasiaca* 107-9 and table.

[137] For all these connections, see *Rech.* i, pp. 278-9.

[138] See *Rech.* i, pp. 274-5.

[139] For the first five columns I refer to Salviat's table, *Thasiaca*, table between 116 and 117; for the later columns to *Rech. i*, Cat. I, between pp. 262 and 263. Salviat divides by colleges, Pouilloux by lines.

There is also nothing in the first five columns that requires the existence of a fourth course 0.385 m. high (Salviat's course VIII), and I expressed agnosticism about the existence of such a course. Salviat replied that in column 8 a block should be assigned to such a course, namely *IG* 287A.[140] (In that case *IG* 287B would be in course VIII of column 9). He did not there state his reasons, but since then he has kindly communicated them to me by letter.

Prosopographical connections show that *IG* 287A should be placed in column 8.[141] Column A of *IG* 287 comprises eleven preserved names and patronymics. Five of these fit column 8 very well:

Ἀκαρνὰν Ἀγοράτου, line 6, is archon in *Rech.* i.34.24. This is the first column of this archon list, which covers the same period as the second half of column 7 and first half of column 8 of the great list (cf. above).

Σκύμνος Ὀρθομένευς, line 7, is the son of Ὀρθομένης Σκύμνου, *IG* 277F. 117, i.e. column 7.6 of the great list, and the grandson of Σκύμνος Ὀρθομένευς of *IG* 277E. 87, column 6.4; cf. *Rech.* i, p. 277.

Δημοσῶν Πυθαγόρευς, line 9, son of Πυθαγόρης Δημοσῶντος of *IG* 278E. 65, column 7.51; grandson of Δημοσῶν Πυθαγόρευς of *IG* 277E. 107, column 6.24.

Πυθῶναξ Πυθώνακτος, line 10, archon in *Rech.* i.34.29 (first column). Fredrich suggested that his father was Πυθῶναξ Λυκίνου of *IG* 277F. 113, column 7.3; see note *ad loc.* Since it is not a recurrence of both name and patronymic, presumably his justification was the relative rarity of the name Πυθῶναξ.

Δημῆς Πυλάδευς, line 11, archon in *Rech.* i.34.27 (first column); son of Πυλάδης Δημεῦς, *IG* 277F. 115, column 7.15.

Three others are clear possibilities:

Ἀριστοκλῆς Ἀμφάνδρου, line 4, archon in *Rech.* i.34.11, possible great-great-great-grandson of ["Αμφαν]δρος Ἀριστοκλέος, *IG* 280.11, and great-great-grandson of ["Αριστοκλ]ῆς Ἀμφάνδρου of *Rech.* ii. 406.7 (first column), if that restoration is correct.[142] 280A is put by Salviat in column 2,[143] and this seems certain; the first column of *Rech.* ii. 406 is certainly in column 3. The date of column 2 is *c.* 510-480, i.e. five to six generations before column 8. The interval

[140] See *BCH* 1983, 184.
[141] Cf. *Rech.* i, p. 277.
[142] See Pouilloux' notes.
[143] *Thasiaca*, table on 109.

is too long for the relationship great-grandfather/great-grandson suggested by Fredrich, *IG* 280, *ad loc.*

Φείδιππος Χρυσώρου, line 5, probably the same man attested as archon in the second half of the fourth century; *IG* xii Suppl. 351.2; 357.7.

Διαγόρης Πρηξιπόλιος, line 12, possibly grandson of Ἰσαγόρης Πρηξιπόλιος, *IG* 277F. 115, column 6.27, since the names are so similar.

Ἀρ[έσ]ανδος Πυθίωνος, line 13, son of Πυθίων Ἀρεσάνδρου, *IG* 278D. 49, column 6.51, is the only case on this block where we can recognize a father-son relationship with column 6. If 287A is in column 8, there was an interval of up to two generations between the father's and the son's service as theoros. But if Πυθίων was theoros young, and fathered his son at a relatively advanced age, and the son was theoros also at a fairly advanced age, such an interval is obviously possible. There are other examples in the list of similar long intervals between the father's and the son's service as theoroi.[144] The case is thus strong for putting *IG* 287A in column 8, and it was already so placed by Fredrich, *IG*, p. 90. As to the course it occupied, since the block is 0.385 m. high, we only need to consider Salviat's courses V-VIII.

One of these courses was very probably occupied by *Rech.* i.36, which Salviat places in course VI. This block contained three columns of names. The surface is badly damaged, and the first column is hard, the second impossible, to read. There are, however, some prosopographical indications of its likely position. In the first column, line 3, Παυσανίης Ἐρξίλλου is the son of Ἔρξιλλος Παυσα[νίου] of column 6.44.[145] This connection would, therefore, point to the placement of the first column of *Rech.* i.36 in column 7 of the great list.

Also in the first column of *Rech.* i.36, Ἀρηΐσκος Σκὺ[θεω], line 10, should be the grandson of the archon of the same name in *Rech.* i.29.9 (second column). That column is dated 420-390, equivalent

[144] As Fredrich noted, *IG*, p. 89. The example there cited was the father Ἀντιφάνης Ναυμάχου, *IG* 277D. 67, column 5, college 15, and the son Ναύ[μ]αχος Ἀντιφάνευς, *IG* 277F. 119, column 7.9. In Salviat's reconstruction (*Thasiaca*, fold-out between 116 and 117, and fig. on 109), the father is in college 15 of column 5, the son one course higher up in column 7. The interval is thus just short of two generations.

[145] This name is preserved on the second list of theoroi, that on white marble, *Rech.* i.27.3 (third column), but was in the great list *IG* 278D. 42, where only the first letter, E, was read; see *Rech.* i, p. 260.

chronologically to column 5 of the great list.[146] This connection would also be good for placing the first column of *Rech.* i.36 in column 7.

In the third column, line 3, Ἐρετριεῦς Κλ[εοφῶ]ντος could be the son of Κλεοφῶν Ἐρετριέως, who is in the second column of the archon list, *Rech.* i.34.37. That column is dated *c.* 315-285, and is equivalent chronologically to the second half of column 8 and the first half of column 9 of the great list.[147] This relationship of father and son, therefore, suits the placement of the third column of *Rech.* i.36 in column 9 or column 10 of the great list.

Also in the third column of *Rech.* i.36, line 8, is Αἰχμόκριτος Πυθί̣ωνος, whose father should be the archon Πυθίων Αἰχμοκρί[του] of *Rech.* i.34.42 (second column). This too, therefore, suits the placement of the third column of *Rech.* i.36 in column 9 or column 10 of the great list.

As we have only four good prosopographical indications of the date of *Rech.* i.36, it might seem advisable to follow Pouilloux' caution, and place it either in columns 7, 8, 9 or in 8, 9, 10 of the great list.[148] But there are other arguments to bring to bear on this problem, architectural and from the succession of names.

There is a hole for the pinch-bar, indicating a joint in the course above, in the top surface of *Rech.* i.36 at 0.46 m. from its LH edge. If in course VI, the first column of *Rech.* i.36 would come beneath *IG* 277F (course V), and the RH edge of block 277E-F is well placed to be above the pinch-hole.

According to Salviat's reconstruction, the third column of *Rech.* i.36, in course VI of column 9, would be beneath *IG* 288A. Although the block 288 is not preserved, and is only known from Miller's publication, the presence of 13 lines in both its columns makes it probable that it was 0.385 m. high.[149] Prosopographical indications are good for placing it in columns 9 and 10:

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[146] See *Rech.* i, p. 276 and Salviat, 'archontes', 238.

[147] For the dating of *Rech.* i.34, see above p. 387.

[148] *Rech.* i, p. 280.

[149] On all the extant blocks 0.31 m. high there is just one column which certainly had 13 lines, *IG* 277D (course IV). Salviat calculated 13 lines for 277C, where little of the inscription is preserved; see *Thasiaca*, table on fold-out between 116 and 117, column 4 course IV; Fredrich gave that block 12 lines. In all other cases blocks of this height contain 12 lines or fewer.

288A. 5, Ἀρισταγόρας Δημάλκευς is son of the archon Δημάλκης Ἀρισταγόρευς of *Rech.* i.34.9 (first column).[150] This connection is good for 288A belonging to column 9.

288A. 6, Αἰχμόκριτος Φρασιη[ρ]ίδευς is possibly son of the archon Φρασιηρίδης Λακεδαιμονίου of *Rech.* i.34.10 (first column); see *Rech.* i, p. 288. If right, this connection would indicate the same position for 288A.

288A. 12, Ἄγνων Τιμύ[λλ]ου is son of Τίμυλλος Ἄγνωνος of *IG* 277G. 140, column 8.2 of the great list. The latter is also attested as archon in *Rech.* i.34.6 (first column). This connection thus indicates strongly that 288A is in column 9.

Architecturally *IG* 288A goes well under *IG* 277H, so it is comfortably placed in course V. If the third column of *Rech.* i.36 is placed under *IG* 288A, we get a good alternation of joints in the two courses, and the second pinch-hole *of Rech.* i.36, which is 1.075 m. from the block's LH edge, is well placed for the LH edge of *IG* 288 to be above it. The architectural arguments in favour of placing *Rech.* i.36 in course VI of columns 7, 8, 9 are, therefore, good.

The succession of names is, however, not so clear. The illegibility of the second column of *Rech.* i.36 means that we are confined to the first and third. The first column begins with two names separated from those following by a paragraph line, showing the start of a new college. The column of the block above should, therefore, end with a single name. *IG* 277F (course V), however, ends with a complete college. To obtain an acceptable succession it is necessary to assume an extra line, which has been missed at the bottom of *IG* 277F or at the top of the first column of *Rech.* i.36. That assumption is very difficult, when both blocks are extant and have been read by expert epigraphists. Note, in particular, that the top edge of *Rech.* i.36 is well preserved.[151]

The third column of *Rech.* i.36 begins with a single name followed by a paragraph line. The block above, therefore, should end with the first two names of a college. In Salviat's reconstruction the block above, *IG* 288A, lies directly beneath *IG* 277H, the position of which is certain, because it belongs to the big joining complex. 277H contains four complete colleges. The 13 lines of 288A must,

[150] For the dating of *Rech.* i.34, see above p. 387.
[151] See *Rech.* i, plate XXIV. 2.

therefore, represent four colleges plus the first name of the succeeding college. *Prima facie* 288A does not provide the two names required by Salviat's reconstruction. Salviat's answer to this problem is to postulate a 14th line in 288A, which was not noticed or copied by Miller. Since *IG* 288 is not preserved, and Miller's testimony is not always comprehensive and reliable, such an assumption could be right; but it is obviously hypothetical.

Thus the succession of names in both the first and third columns of *Rech.* i.36 throws some doubt on its placement in course VI of columns 7, 8, 9. There is a theoretical possibility that it could have stood in Salviat's course VIII of the same columns. Prosopography would remain satisfactory, and the architectural requirements would also be met. In that position only the first column *of Rech.* i.36 could be tested for the succession of names. That column would come beneath *IG* 278E, which ended with a complete college.[152] Thus we find the same problem in the succession of names as was found higher up in the same column, when *Rech.* i.36 was placed in course VI. Since both 278E and *Rech.* i.36 are extant, Salviat's assumption of a missing line would be equally difficult here.

There is also a question about Salviat's placement of *IG* 287 in course VIII of columns 8 and 9. His stated reason for putting that block in the lowest of his inscribed courses was the *vacat* below a complete college at the bottom of 287A. The assumption is that the engraver did not wish to begin a further college, when there was no room to complete it, at the bottom of the column. But that assumption is shown to be open to doubt by the situation in column B of the same block. There was no *vacat* there, and the column ends with the first two names of an incomplete college. 287 is thus different from *Rech.* i.31, a block from the archon list on white marble. On that both columns end with a complete college followed by a *vacat*, and the assumption that the block was in the lowest inscribed course seems well justified.[153]

These considerations show that Salviat's reconstruction is not completely certain. A further complicating factor is that there are other blocks, which seem to compete for a place in the 0.385 courses of column 8 and the adjacent columns. These are *IG* 282 and 286. 286 is 0.385 m. high; 282 is not preserved, but the 14 lines of its sec-

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152 See *Rech.* i, p. 262; the paragraph lines separating colleges are clear.
153 See Salviat, 'archontes', 234-5 and figure on 238.

ond column makes it almost certain that it was of the same height.[154]

286 contains two columns. In column B there are four complete colleges followed by a *vacat.* Column A, on the other hand, has four colleges followed by the first two names of the succeeding college and no *vacat.* In this, and in its other characteristics, 286 is strikingly similar to 287. 282 also had two columns, but in the first only the patronymics were preserved, and not all of them completely.

Prosopographical connections suggest that 286 should go in columns 7 and 8:

286B. 24, Ἀριστείδης Τηλεφάνευς should be the son of Τηλεφάνης Ἀριστείδευς of *IG* 278E. 70, column 7.56, and the grandson of Ἀριστείδης Τηλεφάνευς of 278D. 51, column 6.53. 286B should, therefore, be in column 8.

286B. 15, where only the patronymic Σκύλλου is preserved. Fredrich thought that the *nomen* should be Φιλίσκος, by comparison with Σκύλλος Φιλίσκου of 278E. 67, column 7.53. The combination of the two names is also found (heavily restored) in *IG* 285A. 10, column 4, college 23,[155] and in *IG* xii Suppl. 353.1. Pouilloux, however, did not accept that the *nomen* in 286 was Φιλίσκος; see 'Prosopographie', s.v. Φιλίσκος and Σκύλλος. If we do accept Fredrich's suggestion, 286B should be in column 8.

286B. 18, Ἀριστοκλῆς Σατύρου is the father of Σάτυρος Ἀριστοκλε[ῦς] of *IG* 282B. 28. Ἀριστοκλῆς Σατύρου is well dated, as he is attested as archon in *Rech.* i.34.49 (second column), and *IG* 267.1. The second column of *Rech.* i.34 dates from *c.* 315 to *c.* 285, equivalent to the second half of column 8 and the first half of column 9 of the great list. This connection brings us to the question of the placement of *IG* 282.

In 282A. 3 the patronymic [Νι]κοφῶντος was given the *nomen* Κῆφις by Fredrich, because Νικοφῶν Κήφιος of 286A. 5 should be his father. The supplement was made because the name Νικοφῶν occurs only in these two places, and was accepted in 'Prosopographie', s.v. Νικοφῶν and Κῆφις. If 286A is in column 7, 282A should be in column 8.

In 282A. 10 the patronymic Δικηκράτευς was given the *nomen* Φίλων on the grounds that Δικηκράτης Φίλωνος of 277H. 153, column 9.4, might be his son. The connection is accepted in 'Prosopographie', presumably because the only attestations of Δικηκράτης

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[154] Cf. n. 149 above.

[155] Misprinted as 32 in Salviat's table between 116 and 117.

are with this patronymic. This connection is good for placing 282A in column 8.

Σάτυρος Ἀριστοκλε[ῦς] in 282B. 28 is, as we have seen, the son of Ἀριστοκλῆς Σατύρου of 286B. 18. Together with the other connections noted, this connection points to columns 7, 8 for 286A and B, and to 8, 9 for 282A and B.

If 286 and 282 are so placed, we find two further competitors for courses V-VIII of column 8. Without the dimensions of 282, we cannot judge its architectural possibilities, but 286 could only be placed in these courses at the cost of disruption of the positions suggested by Salviat for *Rech.* i.36 and *IG* 287.

This discussion suggests that the reconstruction of courses V-VIII of columns 8, 9 must remain uncertain. Since, however, the difficulties encountered would be greatly exacerbated, if there were only three courses 0.385 m. high, into which to fit the blocks to be considered, it does seem necessary to accept Salviat's contention that there was a fourth course of 0.385 blocks, his course VIII.

The acceptance of eight inscribed courses opens the way for calculations of the length of the inscribed columns. Salviat calculated that the columns in the first, recapitulatory, part of the list each contained 33 or 34 triads (annual colleges of three).[156] In his later study of the archon list on white marble he calculated that there the columns contained 32 or 33 triads.[157] Salviat's work has therefore confirmed and refined the long-established theory that each column contained the names of roughly a generation.[158]

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[156] *Thasiaca*, 117. I naturally withdraw my suggestion that 24-26 should be seen as the minima; see 'Great List', 113. As for my objection that we do not know if each column contained a complete number of colleges (*ibid.* 108-12), it remains true that only column 3 is proved to end with a complete college, because the only columns where the top course of the triads is preserved are 3 and 4 (*Rech.* ii.406). Column 4 begins with a complete college, but it is not unquestionably certain that 3 did, as we only have the RH ends of its lines, so are without the paragraph lines which mark the divisions between colleges. Salviat's belief that columns 2 and 4, in addition to 3, ended in a complete college (*BCH* 1983, 184-5) is, therefore, not strictly proved. He also adduced the archon list on white marble, where the evidence is better for columns ending with a complete college (see p. 392 above), as a reason for believing that all the columns in the great list ended with a complete college. That seems a good argument. I therefore now accept that it is probable that each column of the great list contained a complete number of triads.

[157] Salviat, 'archontes', 235.

[158] Greek ideas about the number of years in a generation are well illustrated by Hdt. 2.142.2.

If we take it that there were 33 triads per column, we can achieve a close dating of the beginning of the triads, which is marked by the heading, ὑπὸ τὸν χρόνον ὃν οἱ ἑξήκοντα καὶ τριηκόσιοι ἦρχον οἵδε ἐθεόρεον (*IG* 276). We can work back from the college of Ἀντιφῶν Κριτοβούλου, Ἀθήνιππος Κλεολόχου, Κλέοχος Ἀλκίππου. This college presided over a decree (*IG* 263) confiscating the goods of certain citizens of Thasos and Neapolis, and that decree may be confidently associated with the famous decree encouraging delation (ML 83) and with the oligarchic regime which ruled at Thasos from 411 to 407.[159] In his very persuasive discussion of the chronology of these years, Salviat has argued that the most likely year for the college of Antiphon and his colleagues was 409.[160] That is the twentieth college of column 5 on Salviat's arrangement.[161] From Antiphon's college to the beginning of the triads may be calculated at 134 colleges (column 5, 20; columns 2, 3, 4, 33; column 1, about 15). We may assume that there are no missing years, because the Thasians recorded on the archon list times when the office was left vacant (ἀναρχίη); cf. *Rech.* i.28.37 (third column).[162] The calculation leads, therefore, to 543 for the beginning of the triads, which may be regarded as a close approximation to the actual date.

Before that date we have the three so-called irregular colleges, which cannot be so precisely dated. The first college is described (after the heading, Ἀγαθῆι Τύχη[ι οἵδε ἐθεόρεο]ν) as ἐπὶ τῆς πρώτης ἀπαρχῆς; the second as ἐπὶ τῶν δυώδεκα ἀρχόντων οἵδε ἐθεόρεον; the third as ἐπὶ τῆς δευτέρης ἀπαρχῆς οἵδε ἐθεόρεον. There is an important formal criterion for the understanding of these irregular colleges, the paragraph lines. The first college contains 12 names followed by a paragraph line. Of the second we have the first five lines only, and Salviat calculates that there were 11 or 12 lines on the block missing between *IG* 275 and 274, yielding 16 or 17 lines in total.[163]

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[159] See *Rech.* i, pp. 145-6 and the more recent discussion by Salviat, 'archontes', 243-57. I made a gross error, when I wrongly stated that Ἀντιφῶν Κριτοβούλου was one of the archons under whom the delation decree was passed ('Great List', 104).

[160] *Ibid.* 254.

[161] See *Thasiaca*, fold-out between 116 and 117.

[162] This argument is not changed by Salviat's hypothesis that, in a time of *stasis*, an official, who was appointed by the previous regime, was dismissed from office by the new government, who then designated the time he had served as ἀναρχίη. See the discussion in 'archontes', 243-6.

[163] *Thasiaca*, 118.

The paragraph line at the end of the second college is preserved at the top of *IG* 274, before the beginning of the third irregular college. It is theoretically possible that there were paragraph lines on the missing block, indicating divisions within the names in the second college, but the absence of any paragraph line in the first five lines that we have makes it most probable that the second college was marked off by a paragraph line only at its beginning and end. The third college contained 12 names followed by a paragraph line.

We know that the Thasians employed paragraph lines to mark off tenures of office, not only by their use to separate the annual triads of archons and theoroi, but even more strikingly by the change to placing a paragraph line under each individual name on one of the blocks of the archon list on white marble. This shows that there was a time when there were single eponymous archons instead of the usual triads.[164] It is safe, therefore, to conclude from the paragraph lines that each of the three irregular colleges was a whole single college. We lack the evidence to determine how long they were in office, but it is certain that all the men of each group were contemporaries. This is important for the assessment of the prosopographical evidence.

Before that, however, it is necessary to revert to another matter concerning the irregular colleges, on which I differed from Salviat's interpretation.[165] That is the meaning of ἀπαρχή in the headings of the first and third irregular colleges.

It is clear that the twelve archons, like the later 360, are a system of government, but the two *aparchai* are less self-explanatory. Salviat accepted Jacobs' suggestion that ἀπαρχή here also meant a system of government.[166] Jacobs used as an analogy the cognate word ἄπαρχος, which appears at A. *Agam.* 1227 and *Pers.* 327, and means 'commander', 'ruler'. Salviat added the verb ἀπάρχει at P. *Nem.* iv. 46, which some have understood to mean "rules from afar,"[167] and

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[164] See *Rech.* i. 28.30-36 (third column); Salviat, 'archontes', table on 236-7. Salviat assumes that the seven single eponymous archons in our record represent merely the last of a much larger number of such single archons, and that this arrangement probably lasted from the defeat of Thasos by Athens in 463/2 to the restoration of oligarchy in 411; see 'archontes', 239, 245. According to Salviat's arrangement, there were three blocks, now missing, before the second and third columns of *Rech.* i.28; see table on 238.

[165] 'Great List', 113-17.

[166] *Thasiaca*, 120-1; E. Jacobs, *Thasiaca* (Berlin 1893) 15.

[167] Cf. J. B. Bury, *The Nemean Odes of Pindar* (London 1890), *ad loc.*; J. E. Sandys,

proposed to see in the first and second *aparchai* two extended periods, when the mother city Paros put in order the constitution of a newly-founded colony. To this I objected that the noun ἀπαρχή, which appears frequently in our sources from the Archaic period till Late Antiquity, never has any other meaning than 'first fruits' or 'offering' in general. That argument can now be very strongly reinforced owing to our ability to search electronically a very large proportion of Greek literature, inscriptions and papyri. The only meaning of the word attested, in an abundance of evidence, remains 'first fruits' or 'offering', and the meanings suggested by Jacobs and Salviat are completely unparalleled. Doubtful analogies from cognate words cannot prevail against the unanimous testimony to the meaning of the noun ἀπαρχή.[168]

The electronic search also strengthened the second point that I made, namely that theoroi in our evidence are frequently associated with *aparchai*, first fruits or offerings.[169] The natural assumption is, therefore, that the 12 Thasian theoroi "at the time of the first *aparche*," and exactly the same number, be it noted, "at the time of the second *aparche*," were two colleges connected with two important offerings of first fruits, and I maintain that point of view. We can only speculate what the special offerings were, and I have nothing to add to what I wrote earlier.[170]

There is something to add, however, on the prosopography of these irregular colleges and their dating, for here Salviat's work on the archon list on white marble makes some contributions. Six blocks are preserved from the archon list on white marble, *Rech.* i.28-33.[171] Using the heights of these blocks, and following the same principles

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in his Loeb edition of 1915; L. R. Farnell, *The Works of Pindar* (London 1932), 267-8. The analogies for this sense offered by Bury were ἀποικέω, ἀποδημέω. *LS*[9] s.v. ἀπάρχω II, give this sense as probable.

[168] Even though Jacobs' suggestion, 'board of officials', was given in *LS*[9] s.v. ἀπαρχή, 6, and Salviat's idea has appeared in the *Revised Supplement*, s.v. ἀπαρχή (with very inaccurate references to the inscriptions).

[169] See, e.g., the inscription from Priene that I cited, 'Great List' n. 80: *I von Priene* 5.10-15, but no. 42 in the Princeton edition which is used in the collection of inscriptions and papyri on the compact disk. I noted 17 other epigraphical examples of the association; there are probably more.

[170] 'Great List', 117.

[171] *Rech.* i.34 belonged to a second archon list, inscribed on grey marble; see Salviat, 'archontes', 233-4. Although Pouilloux, slightly confusingly, included 34 in his arrangement of the fragments from the archon list (*Rech.* i, p. 276), Salviat, more logically, excludes it from his discussion.

as he had used for his reconstruction of the great list, Salviat proposed a reconstruction of the wall of white marble.[172] Pouilloux had already suggested, using mainly prosopography, the order of the columns in the list, of which he proposed eight, and Salviat's scheme is built on that foundation. To Pouilloux' eight Salviat added a ninth, because of the character of no. 33. The surface to the L of the column of names is not smoothed for inscribing, as it is on all the other blocks. As Pouilloux already saw, this proves that 33 belonged to the initial column of the list, and, as Salviat rightly concluded, cannot belong to any of the eight columns distinguished by Pouilloux. There are thus nine columns in Salviat's reconstruction,[173] which he chooses to number from 0 to 8.

The initial column is numbered 0, in order that the subsequent columns could keep the numbering given by Pouilloux.[174] This decision has the effect of introducing some confusion into Salviat's calculations of the displacements (décalages) in the columns of the archon list and the great list.[175] His procedure is to take an individual present in both lists, and estimate the gap between his positions in the two lists. Thus Ἀριστόπολις Κράτιδος is in college 5 of column 2 in the list of theoroi, but near the bottom of 'column 2' of the archon list (*Rech.* i.31.14 (second column)). So he sees a 'retard' at this point. But since the column in the archon list is actually the *third*, the 'retard' is in fact much greater than he calculates. This flaw affects all the displacements he finds, so I take no further account of this part of his work.

It is the coincidences of the same individual in both lists that are important for chronology and for the layout of the archon list. There are three such coincidences between archons in the second column of *Rech.* i.29 and theoroi in column 5, and one from the same part of the archon list with a theoros near the top of column 6:

Λύδος Λύδου, line 2, is in college 25 of column 5 of the great list.

Βασῖνος Σμίνθιος, line 18, is in college 26 of column 5 of the great list.

Τίμυλλος Νύμφιος, line 21, is in college 28 of column 5 of the great list.

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[172] 'archontes', 238.

[173] He even allows that there might have been more, since we cannot exclude the possibility that there were columns intervening between no. 33 and the other blocks; see 'archontes', 241. He leaves the question open.

[174] See 'archontes', 240-1.

[175] *Ibid.* 239.

Νύμφις Σιμαλίω[νος], line 11, is in column 6.6 of the great list.

There are also four archons in the second column of *Rech.* i.30, who are found as theoroi in column 7 of the great list:

Δείαλ[κ]ος [Θ]εọ[τ]ιμ̣[ίδε]ος, line 1, is in column 7.27 of the great list.

Φίλιππος Νύμφωνος, line 4, is in column 7.17 of the great list.

Νικόδημος Πυθίωνος, line 5, is in column 7.48 of the great list (the name is partly restored, but with good reason).

Πο[λυ]αί[νετος Αἰσχ]ρ̣ίω̣[νος], line 8, (the restorations are justified by the traces in combination with the entry Πολυαίνετος Αἰσχρίω[νος] in the list of theoroi) is in column 7.24 of the great list.[176]

These coincidences are sufficient for Salviat's conclusion that the columns of the archon list were of about the same length as those of the great list. This result enables us to use the secure and close dating of the triads in the great list in order to date the archon list. The archonship of Ἀριστόπολις Κράτιδος is recorded in *Rech.* i.31.14 (second column), that is the last college of the third column (numbered by Salviat 'column 2'). The same man is theoros in college 5 of column 2,[177] that is *c.* 523. If his archonship is roughly contemporary, we have a date of *c.* 520 for the bottom of the third column of the archon list. From that date back to the beginning of the list we have to reckon with virtually all of the third column, all of the second and all of the first. If each column contained approximately a generation, the beginning of the archon list was about a century from the bottom of the third column, and can be dated *c.* 620. A similar result can be obtained from the archon Χάριλλος Ἀριζήλου (*Rech.* i.33.3). He should be the great-grandfather of Ἀρίζηλος Χαρίλλου, theoros in college 18 of column 2,[178] i.e. *c.* 510. The three generations again take us to the later part of the seventh century for the beginning of the archon list.[179]

We cannot use calculation to establish the dating of the irregular colleges of theoroi, so are thrown back on prosopographical indications alone. I showed that such connections make it probable that

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[176] For these connections, see *Rech.* i, p. 268; Salviat is incomplete; 'archontes', 239.

[177] See Salviat's table, *Thasiaca*, fold-out between 116 and 117. Salviat's arrangement of the blocks here is different from that used by Pouilloux for *Rech.* i, Cat. I.

[178] As Salviat, 'archontes', 240.

[179] As Salviat wrote, *ibid.*: "les collèges . . . sur le bloc no. 33 dateraient . . . à l'extrême fin du VIIe siècle."

the second irregular college, "at the time of the 12 archons," was close in time to the beginning of the triads,[180] i.e. about the middle of the sixth century. That dating by itself showed that the third irregular college, "at the time of the second *aparche*," must belong to the same period, and that conclusion is strengthened by the case of Καλλιμήδης Θράσυος. He is archon at a date shortly before *c.* 520 (*Rech.* i.31.3 (second column)). He is also theoros in the third irregular college.[181] That college, therefore, should be placed as close as possible to the beginning of the triads in *c.* 543.

For the first irregular college I could not offer any decisive prosopographical connections in my earlier paper,[182] as Salviat pointed out.[183] The connection between the archon Ῥέων Καλλίνου (*Rech.* i. 33.1) and the theoros Οἴνιχος Καλλίνου of the first irregular college can, however, now be more confidently advanced.[184] The two men are taken to be brothers by the editors of *LGPN* i, s.v. Καλλίνους, 2. The assumption is supported by the rarity of the name Καλλίνους in Thasian nomenclature.[185] If this relationship is right, the first irregular college of theoroi is contemporary with the first preserved block of the archon list, i.e. *c.* 610.

Salviat is right to emphasize the significance and importance of the finding that the Thasians of the first half of the fourth century had in their archives lists of magistrates going back into the later seventh century.[186] Before that, he suggests that they may have been able to date events in their history by using the eponymous archons of Paros. We know that the list went back at least to the time of Archilochus.[187] That suggestion may be right, but on the evidence we have there is a gap between the foundation of the colony and the beginnings of the lists of magistrates. If *c.* 650 is accepted as the date of foundation, that gap is not large, about a generation. This result is important for historians of Greek colonization and Greek historians in general.

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180 'Great List', 117.
181 See Salviat's table, *Thasiaca*, fold-out between 116 and 117.
182 'Great List', 119.
183 *BCH* 1983, 185.
184 Cf. 'Great List', 119.
185 *LGPN* thus reduces to two attested holders of the name the three given by 'Prosopographie'.
186 'archontes', 241-2.
187 Sosthenes' inscription, *Archilochus*, ed. Tarditi, T5, lines 8-9.

It will be seen from the above discussion that I now accept Salviat's proposed number of courses in the walls of the Passage of the Theoroi, but I continue to differ from his interpretation of the irregular colleges. Of the later question Salviat wrote that my arguments did not seem of a nature to close the debate.[188] That I can readily concede; our evidence is not such that any interpretation can aspire to certainty.

There is, finally, one other matter concerning the Thasian lists of magistrates, about which I have not previously written, but which is certainly controversial. Following an unfortunate suggestion by David Lewis, Pouilloux and Salviat identified Λίχης Ἀρκεσίλε[ω], archon at Thasos in 397,[189] with none other than the Spartan Λίχας Ἀρκεσιλάου, who was prominent in the Peloponnesian War and whose death is mentioned by Thucydides at 8.84.5. They hypothesized that Lichas was either given Thasian citizenship, or held an honorific eponymy at Thasos.[190] This identification was taken to show that Thucydides lived till at least 396, which seemed to have an important bearing on the composition problem. The proposed identification was, however, rejected with scorn by Robert,[191] and refuted in detail by P. Cartledge.[192] G. Herman also showed how the homonymy could arise from Greek naming practices in *xenos* relationships.[193] So far as I know, the false hypothesis has never been publicly withdrawn.[194] That seems to me unfortunate, because the theory has continued to appear in discussions of Thucydides' work.[195]

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188 'archontes', 241 n. 11.

189 *Rech.* i. 29.17 (second column). The date of the archonship of Λίχης follows from Salviat's reconstruction, because it can be securely calculated from the easily recognizable year 411; see 'archontes', table on 237.

190 See Salviat, 'archontes', 257-8; Pouilloux and Salviat, "Lichas, Lacédémonien, archonte à Thasos, et livre VIII de Thucydide," *CRAI* 1983, 376-403; "Thucydide après l'exil et la composition de son histoire," *RPh* 59 (1985) 13-20.

191 *Bull.* 1984, 314.

192 "A new lease of life for Lichas son of Arkesilas?," *LCM* 9 (1984) 98-102.

193 (n. 48) 20.

194 Contrast the handsome public withdrawal by G. Vallet of the chronology of the Sicilian colonies proposed by him and F. Villard in *BCH* 76 (1952) 289-346; see his 'intervento', *Insediamenti coloniali greci in Sicilia nell' VIII e VII secolo A. C.*, *Cronache di Archeologia* 17 (1978) 150-2.

195 E.g. S. Hornblower, *Thucydides* (London 1987) 151-2, though he is aware of the objections; S. Flory, "Thucydides' Hypotheses," *TAPA* 118 (1988), 43-56, at 55 n. 30; "The meaning of τὸ μὴ μυθῶδες (1.22.4) and the usefulness of Thucydides' History," *CJ* 85 (1990) 193-208, at 196 n. 10; "The Death of Thucydides and the Motif of 'Land on Sea,'" *Nomodeiktes. Greek Studies in Honor of Martin Ostwald*, ed.

This last topic concerns only an erroneous theory that should be dead and buried. The rest of this paper, however, is not merely retrospective, but treats problems that are current and important, on which there is plenty of work to be done.

Ralph M. Rosen and Joseph Farrell (Ann Arbor 1993) 113-23, at 114-15. Flory knows Cartledge's paper, but underestimates the conclusiveness of the refutation, because of Cartledge's restrained and guarded presentation.

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